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THE PHYSICS OF THE MISSISSIPPI RIVER.

A LETTER TO THE EDITOR OF VAN NOSTRAND'S MAGAZINE IN REPLY TO THE ARTICLE OF JAMES B. EADS IN THE SEPTEMBER NUMBER.

Sir: In the September number of your Magazine, a Review of the Report upon the Physics and Hydraulics of the Mississippi, by Humphreys and Abbot, appeared over the signature of James B. Eads, C.E. He states at the outset that he desires to reach the general intelligent public, rather than scientific readers. Although the Mississippi report was prepared for professional engineers, and its conclusions will stand or fall by their verdict, I ask space in your Magazine to expose some of the errors of this popular review.

Capt. Eads raises two principal issues with our report, each of which will be considered by itself.

RELATION BETWEEN VELOCITY AND SUSPENDED EARTHY MATTER.

The first question at issue is, whether the water of the Mississippi always holds in suspension the maximum amount of earthy matter, which water flowing with that precise velocity is able to support. If yea, then any diminution in the velocity must cause a deposit; and any increase may cause an excavation, provided the bed or banks are of such a nature as to supply suitable material. If nay, then no such results can be rationally predicated.

This question of fact—for theory has nothing to do with it—was conclusively decided in the negative by the long series of observations recorded in the Physics and Hydraulics of the Mississippi. Capt. Eads, with various adjectives which add nothing to his argument, denies the truth of this deduction, and asserts that it is based upon fallacies and blunders made by us in discussing our observations. The accuracy of the latter he does not dispute.

Our analysis of these observations, stated in brief, consisted in plotting two curves, of which the common abscissas were times; and the ordinates, respectively the mean velocity of the river in feet per second, and the corresponding number of grains of earthy matter held in suspension by one cubic foot representing the average for the whole river. If the latter quantity were a function of the former, the forms of the two curves would exhibit a certain symmetry; This was not the case, the amount of sediment per cubic foot when the river was flowing most rapidly being often no more than at the lowest stage.

These curves happen to be very unlucky for Capt. Eads' professional projects; for they overturn the theory upon which he proposes to dispense with levees, to improve the navigation of the upper river, and, in one word, to control

the appropriations which Congress may grant for the lower Mississippi. The world is not wide enough for them and him, and they must be removed from his path. Invoking, therefore, the divinities of Dynamics, Force, Matter, Space and Time to aid in raising a cloud of confusion over a very simple question of mathematics, he makes a desperate assault upon our method of reasoning. Not to follow him through several pages of this style of writing, which, although perhaps effective with the popular audience to which he appeals, will be passed over by professional men, let us examine his final conclusion. It is, that the ordinates of the sediment curve must be multiplied by the corresponding discharges of the river, before they can be compared with those of the mean velocity curve.

The absurdity of this proposition has been so clearly pointed out by Mr. R. E. McMath, C.E., in the *Engineering News* for February 28, 1878, that it is needless to repeat the elaborate algebraic demonstration by which he arrived at the conclusion that "mathematically this step is a blunder which would disgrace a boy in the junior class of our High School."

Suffice it to say, that the quantity under discussion is not, as Captain Eads argues, the total *mechanical work* performed by the river, which of course depends upon the number, as well as upon the supporting power, of all the cubic feet of water in motion; but *the force* which neutralizes the gravity of the earthy matter held in suspension. If this force be less than gravity, a deposit must occur; if it be equal to gravity, the water will be charged to its maximum supporting capacity with earthy matter; if it be greater than gravity, an additional amount of earthy matter may be added without causing a deposit. Now, Capt. Eads' theory confessedly requires that the water be always charged to the maximum capacity due to its velocity. The point, then, for observation to determine, is, whether there be any fixed relation between the earthy matter suspended in a unit of volume, and the horizontal velocity of that unit of volume. If any fixed relation existed between these quantities for the Mississippi, our curves would reveal it; but as they show the reverse, Capt. Eads' theory falls to the ground.

That the figures plotted by us correctly represent the quantities in question, is evident when we consider what they are. The mean velocity is the mean of the horizontal velocities of every cubic foot of the river. The amount of sediment was measured by collecting samples of water from three stations—one, 300 feet from the East bank, where the high water depth was 100 feet; another in the middle, where the depth was 100 feet; and the third, 400 feet from the West bank, where the depth was 40 feet. The total width was about 2400 feet. One hundred grammes of river water were collected daily, Sundays excepted, at surface, mid-depth and bottom, at the two deep stations; and at surface and bottom at the other. The figures plotted in our curves represent the mean weight of sediment per cubic foot for the entire river, computed by averaging the results from the eight stations. This averaging is legitimate, because the distribution of earthy matter held in suspension is remarkably uniform throughout the river, as is proved by the following yearly totals:—

DISTRIBUTION OF EARTHY MATTER.

Station.	High Water Depth	Total grammes collected in one year.		
		Surface.	Mid-depth.	Bottom.
Near East Bank.	100	15.302	17.552	17.880
Near Middle....	100	15.156	18.977	19.538
Near West Bank	40	13.845	—	20.070

The bearing of these figures upon Captain Eads' theory as "modified by depth" is too plain to need comment.

To recapitulate the foregoing views in more concise and mathematical language, the problem to be studied experimentally is, whether:

$$S=f(V).$$

In which S denotes the force which, opposed to gravity, maintains the earthy matter in suspension; and V is the horizontal velocity of the volume of water supporting said earthy matter. Now let us see what our critic proposes to do. If

he were not so bitterly in earnest about the problem, his blunder would be funny. He actually multiplies one member of the equation of which the truth or falsity is to be experimentally decided, by the *discharge*, which is nothing but the product of the area of cross section (a) by the mean velocity, (V); and leaves the other member unchanged, giving:

$$Sa\mathrm{V}=f(\mathrm{V}).$$

And he then proceeds to congratulate himself, and to apply adjectives to us, in honor of the surprising discovery that, after he has introduced V into the first member, that member thus modified can be proved to be a function of V!

It would be easy to point out many other minor errors in this division of Capt. Eads' review, but after this exposure of the fundamental fallacy upon which his whole argument rests, it would be wasting time to do so.

Our position in the matter simply is, that the earthy matter which the river holds in suspension is chiefly that brought to it in suspension by its tributaries; and that if the amount they supply at any time be less than the velocity would support—as is usually the case in floods from the comparatively clear tributaries, like the Ohio—the water remains under-charged. Capt. Eads' claims:—"If the reader will bear in mind that the water is charged with sediment according to its velocity, and that it flows through a bed of precisely the same kind of material it is carrying in suspension, and that if its velocity is increased it will take up a greater charge from its own bed, or if its current be slackened it will drop some of its charge in the channel, and add to its bed, he will understand the important part which the speed of the current performs in the problem." Perhaps in thus committing himself, Capt. Eads' did not know that at Columbus, Ky., 20 miles below the mouth of the Ohio, the waters on the East side of the channel, which have issued from that river, although moving side by side, and with equal velocity with those from the Missouri on the West side, contain only about three quarters as much earthy matter in suspension. This fact, stated in the Mississippi Report, is proved by eight months' daily observations at Columbus. It would be ill-natured to wonder whether, if Capt. Eads' had noticed this ugly fact, he would have announced that, according to his investigations, these data "bear excellent testimony to the care with which Messrs. Webster and Fillebrown conducted the experiments at Columbus."

BED OF THE MISSISSIPPI.

The second main issue which Capt. Eads raises with our report, is in respect to the nature of the bed of the Mississippi. In this matter, he sets up a man of straw, and after overturning him, claims by so doing to have refuted our views.

As an example of the reckless misstatements of facts which characterize the whole review, I quote the following paragraph:

"By reference to pages 135 and 137 [Mississippi Report] it will be seen that this extract contains an astonishing exaggeration. Instead of *three years*, the current and sediment observations only occupied *eight months* at Columbus, and *one* year at Carrollton.

"When we remember that the junior author of the report on the Mississippi River was a prominent member of the Levee Commission, and that the senior author, as Chief of Engineers, warmly endorsed its report, it is difficult to reconcile this careless statement with the unusual scientific exactness which required four decimals to record their measurements of the current."

Turning to the page next preceding the one mentioned, we find a table giving in detail the results of the *two* years of sediment observations made by a party of the survey at Carrollton; and on page 142, another, giving the details of a continuous series of nearly three months' observations, made by Prof. Riddell. These with the eight months' record at Columbus, sufficiently sustain the accuracy of our language.

After such an exposure, it is sufficient without devoting space to other instances of similar inaccuracy in this Review, to ask the reader to apply the old maxim, "ex uno disce omnes," and take the precaution to refer to the report itself, before accepting as trustworthy Capt. Eads' statements respecting its contents.

This precaution is particularly necessary in order to understand our real

position in relation to the bed of the river. Capt. Eads endeavors to convey to the reader that our views rest solely upon a limited number of soundings in the river itself, and that all the information we possessed on the subject is reported in full in Appendix C. Neither of these ideas is correct. It is nowhere stated, nor is it true, that Appendix C describes every sample brought from the bottom. In the work done by me, personally, in which I used one of the transits on the bank, it frequently happened that when the boat brought the specimens for inspection, some uncertainty would exist as to the exact soundings in which individual specimens had been secured. In such cases, the samples were not bottled and no specific entry was made, but the general nature of the bottom was carefully noted, and this information was used in the final report. Capt. Eads might as well demand that a botanical collector should exhibit specimens of a plant gathered from every locality in which he discovered it, as to apply such a criterion to our work.

But if this kind of criticism is unfair, for a still stronger reason is the unfounded assumption that our views are solely based upon soundings in the river itself. In the report and appendices will be found stated at length many facts bearing directly upon *the geology of the region traversed by the river*, and which, from their wide range and perfect accordance with each other, will, it is believed, have great weight with any unprejudiced mind. Capt. Eads' whole argument on this point is that of a lawyer endeavoring to raise quibbles, rather than that of a judge stating the truth in an impartial manner. Readers who desire to study the subject should refer to the report itself, where the arguments are stated in as concise language as can well be used. They proved convincing to every member of the Commission of Engineers to which the subject was officially committed by Congress; and that they do not harmonize with Capt. Eads' wishes and theories, is our misfortune, not our fault.

One other point. Lest Capt. Eads' assertion that the shoaling of the bed of the river below Cubitt's Gap proves that crevasses cause a deposit in the channel, be accepted as true reasoning, I will simply say, that the location of this break, just above the head of the Passes, introduces anomalous features, which prevent occurrences there from having any legitimate bearing when applied to the river above. The great Bonnet Carré Crevasse, above New Orleans, has been open since April, 1874, discharging an immense volume of water at every high stage of the river; and yet no well-marked shoaling has occurred below its site. Capt. Eads may perhaps venture to deny this statement, which is fatal to his theories; but the facts set forth in the Report of the Commission of Engineers, ordered by Congress to investigate and report a plan for the reclamation of the alluvial basin, fully confirmed by later surveys, speak for themselves, and establish my position.

THE ST. LOUIS BRIDGE.

I must now refer to the single paragraph which has called forth this reply to Capt. Eads—his other statements, I think, might have been safely left unanswered, to be judged by professional men.

It must be apparent to every reader, that the personal animosity constantly exhibited by Capt. Eads toward the Chief of Engineers, and which marks every page of this review, unfits him from taking a fair view of the subjects discussed. This has led him not only to misrepresent the real issues, but also in the following paragraph, to introduce one entirely irrelevant:

"A few years ago the Chief of Engineers of the United States Army, being equally as well convinced that the steamboat smoke pipes were, like the bed of the river, unyielding in their nature, and that they were too high to pass under the bridge which spans the Mississippi at St. Louis, accordingly recommended that a canal with a drawbridge, through the bridge approach, to accommodate these unyielding smoke pipes, should be dug round the end of the bridge in the ancient geologic blue clay in Illinois, at a cost of over three million dollars! The fact that the river was proved by 'a glance at the two diagrams' to be always undercharged with sediment, was an assurance that the canal would be a success and would not silt up. But Congress did not look with favor on this plan. Doubts as to the unyielding nature of

the smoke pipes were openly expressed, and while the canal plans and estimates were being prepared the lucky discovery was made that the whole difficulty could be avoided by putting hinges in the pipes; and so three millions of public treasure were saved, and the commerce of the river now flows under the bridge without let or hindrance."

This paragraph is full of errors of fact, which might naturally mislead any one not familiar with the subject. The truth is the following:

On account of the strong complaints of navigators of the Mississippi River against the bridge which Captain Eads was building at St. Louis, a Board of Engineers was convened by the Secretary of War, to examine and report upon the matter.

At one of the public meetings of the Board after they had become satisfied that the complaints of navigators were well founded, and while they were discussing a remedy, a member stated that the case of the Louisville bridge was somewhat similar, and that he had provided for high water navigation by a channel cut into the canal bank at the South end of the bridge. He suggested that a similar plan might obviate the trouble at the St. Louis bridge, and proposed a cut into the shore behind the East abutment pier. The Board provisionally adopted this plan, and recommended that the local engineer officer be charged with the duty of working up the details and ascertaining the cost.

At a subsequent meeting of the Board, the local engineer officer reported as the result of his investigations, that the cut with its accessories would cost $922,436, and that the annual expense of operating the draw and keeping the cut open would be $15,000, which, capitalized at six per cent. would be $250,000, making a total cost of $1,172,436, (not three millions).

There can be no question as to Capt. Eads' knowledge of these facts, as he published a pamphlet in which he criticized this report with great bitterness.

From his statement above quoted, it would be inferred that engineer officers had never heard that steamboat chimneys could be lowered, and that as soon as the knowledge that such a thing was possible reached the ears of Congress, it at once decided not to interfere with the bridge.

In the first place, every boat engaged in commerce on the Ohio River, is provided with hinges, and this has been the case for many years. Even the little surveying steamer, the Major Sanders, in charge of one of the members of the Board, was provided with hinged chimneys. Evidently, therefore, they were no novelty to him, and the same remark can be made with regard to the other members, all of whom had had much experience in Western river navigation.

In the second place, the Board in their report stated that one of the objections to the St. Louis bridge was the following:

"The height under the lower arch is so small that a large proportion of the boats which will have occasion to pass under it must lower their smoke stacks at all, or nearly all, stages of the river; while many of the larger boats will not be able to pass under it during the higher stages, even with their smoke stacks down."

And yet Capt. Eads says that the discovery that steamboat pipes could be hinged, was made "while the canal plans and estimates were being prepared!"

The report of the Board contains no reference to "ancient geologic blue clay in Illinois;" indeed, there was no allusion whatever to the nature of the material of the East St. Louis wharf, unless an item in the estimate calling for "earth excavation (dredging)" at thirty cents per cubic yard can be considered as such. Nor was there the slightest reference to the question whether the river water was over-charged or under-charged with sediment; nor was there any "glance at the two diagrams." On the contrary, it was well known that the cut would probably silt up if left alone; and an estimate of $10,000 per annum was included for the specific purpose of keeping this cut open and the walls in repair.

Finally, *the only individual connection of the Chief of Engineers with these matters was, that the reports of the Board were submitted to the Secretary of War through him*, as required by the Army Regulations, and were formally approved with the recommendation "that the matter be submitted to Congress at its next

session for such action as in their judgment may seem to be necessary."

The above facts furnish such a commentary on the paragraph I have quoted, that their simple presentation is more severe than any language which could be employed. I leave the reader to draw his own inferences—mine being that any future attack of this nature from Capt. Eads, cannot be held to require notice.

HENRY L. ABBOT,
Major of Engineers and
Brevet Brig. General.

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SANITARY SCIENCE IN THE UNITED STATES—ITS PRESENT AND ITS FUTURE.

BY ALBERT R. LEEDS, PH.D.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

IT is generally conceded, I believe, by scientific laborers in this country, that we have been more fertile in invention than discovery. We owe to older nations a constantly increasing debt of obligation for those initial germs of thought which have fructified into new sciences, while we may, at the same time, ask a generous acknowledgment of the merits of many inventions which have opened up new fields of employment to thousands. Sciences which promise much for the improvement of the daily condition of mankind, and have in them a side largely practical are sure of welcome in our midst. Such a science is pre-eminently the one under consideration. It gathers into one the teachings of all other sciences, so far as they bear upon private and public health, and makes these teachings practically operative in the promotion of human welfare in this country. It grew into prominence during the war of the Rebellion, when the work of the Sanitary Commission was made co-extensive with every army camp and army hospital. Its principles have been expounded in Sanitary Associations formed in many States and in smaller communities. These have led to the formation of State and City boards of health, clothed to a greater or less degree with executive functions.

Every epidemic has fastened popular attention upon the subject, and what before was taught in book or lecture-room has been rehearsed in a thousand forms in the newspaper. In this present yellow fever plague more than twelve thousand people have perished, probably not less than sixty thousand have convalesced, and two hundred millions of dollars would not represent the aggregate pecuniary loss. During its awful course universal interest has been felt in the cause and prevention of this and similar diseases, a homily on private or public hygiene has formed a prominent feature of the daily paper, and this interest has culminated in the offer made by a lady, already widely known by her munificence in the cause of science, to defray the expenses of a commission of inquiry composed of sanitary experts. We believe that this is as it should be, and that in sanitary science this country is taking a foremost place, because popular sympathy and popular knowledge are running almost abreast of the science itself. The proper execution of sanitary laws demands the free and intelligent co-operation of the individuals; a system of enactments, however skillfully framed, and however supported by a strong central authority, alone will not suffice. Not only would it appear alien to the genius of our institutions, but also a mode ill-suited to attain its object, if a Health Department were added to the other departments of State at Washington. No one would deem it possible for such a department to legislate pure air, pure water and pure food into use throughout the nation. On the contrary, it would appear far wiser to leave such legislation to each State, and in the State as far as possible to each community, recognizing that the popular agitation and knowledge

requisite to obtain health laws is the best guarantee that they shall not afterwards remain dead letters. What I have to say concerning the present and future of Sanitary Science in this country is mostly the record of what such communities have already done, and of what ideas are now growing up in their midst, and which, ere long, will bear fruition in new laws and movements.

I. VITAL STATISTICS.

We shall begin at the foundation stones of exact sanitary science—Vital Statistics. To appreciate the advance in this direction, we have only to compare the condition of our greatest city, New York, at the beginning of this century with its present. At that time it had no registration even of deaths. The first "Bill of Mortality," as it was called, extended from November 1, 1801, to January 1, 1803, fourteen months. So little accuracy in the nomenclature of diseases was thought of or expected, that in this report the people are said to have died of "flux," "hives," "putrid fever," "rash," "lingering illness," (which certainly was not a rash performance), "stoppage," "breaking out," "fits," etc. The first reliable report was that made in the year 1866, after the organization of the Metropolitan Board of Health. In the second annual report in 1867, the beneficent results of the institution of the Health Board, and of the sanitary reforms executed under its superintendence, were shown by the fact that there were 3,152 less lives lost during the first year of its administration as compared with the preceding year. The report, moreover, showed that this gain had been mainly in the chances of life at the adult ages, and in the districts where the greatest amount of sanitary work had been performed. The causes of insalubrity affecting infant mortality were not yet within control. In the year 1868, the work of registration was extended and specialized in such a way that comparisons could be made in the death-rate between portions of the city occupied by a degraded and overcrowded population, and those more favorably situated, whether in point of natural advantages or in the character of inhabitants.

This specialization enabled the sanitary inspectors to judge of the value of their labors, in the matter of cleansing and disinfection. In fact, they had lowered the death-rate in certain of the most wretched wards below that in some of the best, sanitation in these latter having been omitted. The registration extended also to the effect of modes of living upon the death-rate, and in this manner pointed out the necessity of controlling the excessive mortality in tenement houses. That health reports when promptly and intelligently used might be effectively employed in the prevention of disease, was shown by the use of the returns made during the last two weeks in July, 1868. The Registrar, apprehending that the infective quality of Asiatic cholera might prove to be present in the rapidly fatal diarrheas then prevalent, sent warning to the Surgeon-General of the United States Army, in consequence of which the General in charge of the recruiting and transportation of troops, ordered the immediate suspension of that branch of the army service in New York. Valuable illustrations of the relation existing between damp houses and pulmonary consumption were obtained by selecting certain wards of the city, and forming maps in which every death from phthisis for a number of years was marked on the chart opposite the locality of its occurrence. The evidence so obtained pointed to an excess of consumption at the lowest levels, and in two of the wards to a crowding of fatal cases of this disease in localities unusually damp and in rainy seasons flooded, although these dwellings differed in no other respect from the average of the ward. The results obtained in this manner were deemed so valuable that the registration was applied to each house in the city. In this way excessive mortality in any locality, or from any special class of diseases, become known at once to the sanitary inspectors.

With regard to the registration of marriages, improvement was more difficult. The system of registration, expecting a voluntary support from clergymen and civil officers concerned, could secure very partial returns, and it was only by diffusing information through the press and the lavish distribution of circulars that the accuracy and completeness of the returns in this respect could be improved. An inquiry into the number of births

registered, as compared with that which the Board had reason to believe occurred, revealed a deficiency in the registry of 65 per cent. It has been stated by an American writer on these topics that "it would be impossible for a large portion of the adult men and women born in the United States to prove by any public records or other legal documents, that they were legitimate offspring, with a natural right to the name they bear, or even that their parents were ever married."

The system of mortality registration was gradually improved until the returns made in the year 1871 were probably nearly perfect. When compared with the mortality in other cities, this accuracy told against New York, for while its death rate was 28.6 per thousand, that of St. Louis in the same year was reported at 17 in a thousand; of Rochester, 16; Buffalo, 14, and Jersey City only 7. In the matter of marriages and deaths, the increased knowledge among clergymen, physicians and others, on whose voluntary co-operation the registration largely depended, had resulted in an apparent increase in the annual marriage and birth rate, but still the number of births returned was probably less by 10,000 than the true. In the following year the board instituted suits against these parties, which had a beneficial effect, but it became evident that nothing short of important changes in the law would secure completeness.

I have been thus particular in narrating the history of vital registration in New York, because this city was the first to undertake a reform, and because its reports were the first which attempted to keep abreast with the developments of sanitary science, and to diffuse this knowledge broadcast. The course of legislation on these points is one which every city and State has gone, or is going, through. In reference to New Jersey the facts are so fresh that I scarcely need recall them. At each meeting of the New Jersey State Sanitary Association, since its origin three years ago, the inaccuracy and worthlessness of the State Vital Statistics were conclusively shown in the reports of the committees on this subject. The association formulated a protest, and appointed a special legislature to memorialize the Legislature. By these means, and by the efforts of the State Board of Health, public opinion on the subject was awakened, and so far educated that, during the winter just passed, a law was enacted which gives to New Jersey one of the best systems of registration as yet devised in this country. It has incorporated in it two features to which its peculiar excellence are due, and which should be universally copied; 1st. That of issuing *burial permits* only after registry has been made by a properly qualified person. 2d. The returns are made to an expert, who collates them in accordance with the views of the most eminent authorities, and draws from them their most important teachings for immediate and very practical application.

II. REGISTRATION OF DISEASE.

We must not rest content, however, with the returns of mortality—we should advance to the registration of disease. This is practicable, and if not in all, yet in that large class of diseases in their nature preventable, of universally acknowledged utility. We do not delay until a street brawl becomes a riot, before notifying the magistrate and securing police aid, neither should we wait until diphtheria, typhoid, etc., become epidemic before sending intelligence to the custodians of the public health. But this is not all to make their knowledge of public utility, these custodians must be invested with adequate powers. At present there is little more expended upon the whole work of the Board of Health of the State of New Jersey during an entire year than the pay of two policemen. Its members labor without remuneration for the Sanitas Publica. Their power is mainly the educational impetus of just ideas, forcibly expressed. There are many ways of promoting sanitary reforms, but none it appears to me so practical as that of giving to the Health Board the money, means and men to register diseases, to investigate their causes, to suggest and promote their remedies, and not unfrequently to bring offenders to suitable punishment.

III. THE SANITARY LEGISLATION.

There is a source of danger, as this last summer has strikingly shown, which cannot be warded off by sanitary legislation when limited to a few of the States. If those States, which are the seats of

yellow fever, year after year, do not provide efficient precautions to suppress or control the epidemic, it will annually invade other localities following the lines of travel, and spreading northward to the Mississippi basin. We have recently seen the alacrity with which more favored communities came to the relief of those afflicted with the epidemic. Help of every description was sent until the bountiful public was asked to hold its hand. While the terrible plague lasted, was not thought a time for good advice, but for good deeds. Now that the danger is over, the time has arrived to avert similar visitations in future. Does it appear unreasonable to ask for the most skillfully devised sanitary regulations in localities where such a pestilence may germinate? Recent events have elicited a vast deal of discussion as to the origin of these epidemics and the modes of combating them. There is want of harmony, however, in all points but this, that some of the factors, which are concerned in originating the disease, are within human control, and prevention therefore, is the duty of the authorities where the disease germinates. Those involved in the consequences of neglect of these duties, however remote their homes, have a right to ask reform. This agitation should not be allowed to die out with the pressure of the calamity which aroused it. It should be continued until every one of the States has an efficient health code. At present the majority have either none or very deficient health laws. Massachusetts has strikingly shown its general enlightenment by being the first State to have an efficient health board and a wisely-devised code of sanitary legislation. New York and Pennsylvania have neither, though strenuous efforts have been made by public-spirited individuals to do away with the stigma. In the West, Michigan has been distinguished by the excellence of its sanitary legislation, and the voluminous and valuable publications of its State Health Board.

But Arkansas and Missouri are sadly deficient, and the case is even worse in Iowa, Kentucky and Indiana. Some attempts to supply the most pressing wants have been made in Florida, and in North and South Carolina, and health laws are not entirely wanting in the statute books of New Hampshire, Maine and Rhode Island. The necessity of educating the people in each State, before the requisite legislation is secured, will require a considerable period to elapse before all the States have systems of laws in accordance with modern knowledge. In the meantime, in the name of all those good men who have perished, and as an acknowledgment of the nation's charity, let the plague-stricken States of the Gulf and the Mississippi basin lose not a day in adopting the wisest precautions, experience and investigation can offer. Struggling as we are in this country to have the importance of sanitary legislation generally recognized, the progress made in some directions is highly encouraging. It is probable that no community will take steps to learn what is essential to its health before it has suffered from lack of information. To the distress of London the world owes those great works of the Royal Commissioners on Water Supply and the Pollution of Rivers, which are the repertory of the best knowledge on these topics. The manufactories of England have made it necessary for the government to take cognizance of aerial impurities, and which has been done in that country towards establishing a chemical climatology. Similarly the pollution of the Passaic by the manufacturing towns above has caused inquiries to be set on foot, akin to those referring to the pollution of the Thames, and has given rise to extended inquiries into the methods and aims of water analysis.*

An attempt was made to deprive the inhabitants of New York of some of their public parks and occupy them with buildings devoted to military and other purposes. The more public-spirited citizens came to the rescue, and through the influence exerted, a Public Parks Association, and other means preserved the open squares as breathing places and pleasure grounds. The association recognized as its principle of action, that to preserve the parks they must be improved. The proposition was made and eloquently advocated by Dr. Seguin, that the physical

* Report to the Board of Public Works of Jersey City. Professors Hurtz and Leeds, Analytische Bertrage, Prof. Leeds, Zeitschi für Anal. Chem., 1878. Recent Progress in Sanitary Science, *ibid.* Annals of the Lyceum of Natural History, N. Y., vol. xi., 1878. Water Supply of the State of New Jersey, *ibid.* Journal Franklin Institute, March and April, 1878.

as well as the spiritual well-being of the citizens at large would be powerfully augmented by making the public gardens *out-door schools*, supplementing the in-door school system by that in which they are lamentably deficient, an education in the phenomena of plant and animal life. A beginning in this direction has been made in the Botanical and Zoological Museums of the Central Park of New York, and in the Fairmont Park in Philadelphia, but these are remote from the centers, and the objects of study should be placed where they could constantly appeal to the eye. The hygienic value of gratifying the sense of beauty as well as satisfying the requirements of use is more and more recognized. The first society on this side of the water organized with this object, was the so-called "Land Hill Association," of the village of Stockbridge, in Western Massachusetts. After twenty years of activity, the result has been to produce a village of exceeding loveliness. Thousands of trees have been planted out along the road-sides. The village cemetery, formerly neglected, has been surrounded by an exquisitely kept hedge. Monuments have been erected to the memory of villagers whose subsequent achievements have made the place of their birth illustrious. Prizes have been offered for those who labored most efficiently to improve the health and beauty of their native town, and for these prizes the poor as well as the opulent contend. In fact the neatly kept side-walks, the attractive gardens, the pretty cottages of the poor are a better indication of what healthy pride can do for a community than the trim lawns of the rich. I need not add, that in a community where these things, which add grace and beauty to the daily life have been done, the more important works of water-supply, drainage, sewerage, etc., have not been left undone. Similar associations have sprung up throughout New England. In Williamstown the villagers have thrown down every fence, and this most picturesque of country villages is a beautiful park, through which the houses of the inhabitants are scattered. Some of the towns of New Jersey have caught the spirit of improvement, and recently I heard that Boonton, for which nature has done so much, was about to turn this bounty to account, and fill the houses, which a reverse of industrial prosperity had left vacant, with tenants attracted by its added charms of nature and art.

IV. VENTILATION.

In the matter of ventilation, a considerable advance on the whole is to be noted—in other words, the percentage of failures to successes, in cases where methods of ventilation, for the time being in vogue, have been tried, is slowly growing smaller. The volume of scientific literature, founded on our increasing knowledge of the properties of materials, of gases and of heat, grows more rapidly than the generally accepted rules by which the art of ventilation is to be practised. It is noteworthy that there are few persons who do not regard themselves competent to arrange the ventilation of an ordinary building, and it has hitherto been left largely to the builder, the vestryman and the school-trustee. This should not be the case. What advance has been made is mainly due to the specialization of this kind of professional labor—the formation of a class of engineers who devote themselves exclusively to problems of this character, and who have fought their way into practice by successful work accomplished. The architect submits his plans to these specialists, who add to them the requisite details of heating and ventilation. It would be a great step in the interests of sanitary science if the school or hospital trustee would not think it devolved upon them, as a portion of their office, to become for the time being an authority upon ventilation, and if they were, as a proceeding of sound economy, to relegate this duty to persons properly qualified. As a matter of fact these qualifications are obtained at a considerable expense to the community, for in this stage of the art of ventilation, there being no authority universally recognized and but few generally conceded rules, every sanitary engineer goes through a similar series of experiments and failures before he arrives at a reasonably successful method in practice.

As far as I can learn there appear to have been great successes as well as great failures, whether the system of ventilation by aspiration has been resorted to or that by propulsion. At the present

time many authorities of note have declared in favor of mechanical ventilation. And yet in a number, I might say in most of the asylums and hospitals in this country where fans have been introduced, they are now standing still. The Roosevelt Hospital, for instance, in New York, where the fan, after having been put in operation, was run backwards and was run so for months. It is now stopped. This is one fact of many, which would make us chary of affirming positively that either system is the better. Probably both discreetly applied yield good results, and in their skillful application and not less the faithful supervision of the ventilating apparatus after introduction are good results to be sought.

V. PHYSICAL EDUCATION IN SCHOOLS AND COLLEGES.

Progress in this direction has been initiated at our highest seminaries of learning, and is slowly working its way downward through our educational system. I do not refer to so-called athletic sports, although these had not attained to much prominence in our colleges prior to the year 1850, but to the introduction of physical exercise and instruction on hygiene as a part of the college curriculum. This, so far as I am aware, was first successfully accomplished in Amherst College, and now, after a trial of nearly twenty years, is still regarded as an indispensable adjunct of the college course. The dignity of this department of instruction is emphatically recognized by appointing to it only distinguished members of the medical profession and including them in the college faculty on the same footing as the other professors. It is made their first duty to know the physical condition of every student, and to see that the laws of health are preserved by them. In case of sickness, the students apply to this officer for a suitable certificate, which excuses them from college duties, and are put in the way of obtaining suitable treatment. The statistics of the bodily condition of the students are regularly and frequently secured, and are supplying a collection of "physiolgical constants," which are of growing interest, and supply practical helps in determining whether the student's physical condition is within the bounds of health, and whether their development from time to time is normal or otherwise.

All the classes are required to attend the gymnasium exercises four times a week, and the regularity and faithfulness in this is made an element in collegiate standing. The performances are accompanied with music, and arranged to give full play to the animal spirits. This and the advantages personally experienced by the students, have conspired to make the gymnastic fully as popular and well-attended as the literary exercises. Finally, the intelligent co-operation of the students is secured by instruction upon the means of preserving health, physical and mental, with supplementary lectures upon human anatomy and physiology. Prof. Hitchcock writing* of the chances of life of the young men under this hygienic discipline as compared with men of the same age elsewhere, says "it is regarded as an established law that the chances of life grow less and less from about the fifteenth to the twenty-third year, and the rate of decrease is very rapid. But the tables of health, as kept at Amherst College, show that there is an improvement in health from year to year through the course, the ages being from nineteen to twenty-three. For taking the number of sick men in the freshman class as unity, the number in the sophomore year is 0.912, in the junior, 0.759, and in the senior but 0.578, the percentage of sickness during the college course diminishing to nearly one-half.

In the light of this successful experiment continued for a period of twenty years, it is not premature to urge upon colleges generally the formation of a similar department; and for my own part I see no method of raising the character of public school instruction so effectually, than that of giving to the *physical training* of the children a very prominent place.

VI. HEALTH RESORTS.

The growing facilities each year for travel are steadily increasing the number of citizens who visit the country, the sea shore and the mountains. A salubrious village is frequently transformed into a center of pestilence merely by such in-

* Hygiene at Amherst College. Prof. Hitchcock, Amer. Public Health Assoc'n.

flux of strangers, the entirely natural means of disposal of refuse and excuta no longer being adequate, and the artificial methods not being provided until an outcry due to disease is raised.

In no way, however, is the growing intelligence on sanitary matters more strikingly shown than by the extreme sensitiveness of pleasure seekers to the salubrity of summer resorts, in respect to water, sewerage and drainage. Of the multitude of illustrations I need but speak of Bethlehem, in New Hampshire, a beautiful village situated 1,700 feet above sea level, and so renowned for the purity of its atmosphere; that of the 40,000 hay fever patients, whom Dr. Beard has calculated exist (and hay fever patients say that life to them is only a tolerable existence) in this country, several thousands annually spend part of the summer there. Its popularity increased in a few years so rapidly that a crowded village soon arose, and during the summer of 1877 an outcry was made concerning drainage. The towns people realizing that the reputation of salubrity being the wealth of the town, steps must be taken at once to preserve it. They did so and during the present summer the influx of visitors has been greater than ever.

These two stages in the growth of a summer resort—its sanitary degradation and subsequent rehabitation—cannot be witnessed in every phase of their development, along the entire coast-line of the State of New Jersey. This great sea-side resort, a hundred miles in length, stretching from Sandy Hook to Cape May, is rapidly growing into an almost continuous city. It harbors each summer a vast multitude from our two Metropolitan cities, and from the middle and middle Western States. Even as a Sanibarium in winter, the physicians of Philadelphia, during the past lustrum, have recognized the great advantages that were pointed out by Dr. John Toney nearly a half century ago, and are sending their patients in need of change of air to Atlantic City and neighboring points. The arid expanses of its sandy shore have become in this way one of the principal sources of income to the State. Would it not then be a highly remunerative policy for the State to maintain their attractiveness. As a fact, nothing of a preventive nature was done. The shallow pits, which provided surface water for drinking and other purposes when the population was sparse, were multiplied when the visitors came by many thousands. Malarial and typhoid fevers were rife in spots which the sea-breezes visited every day. Only with the consequent suspicion and public alarm which threatened to empty them of their patronage, did these places tardily move in the matter of adequate sanitation, and now the universal introduction of cemented cisterns and the diurnal removal of garbage under the stringent regulations of local Boards of Health, attest the purpose of the great sea-side sanitarium to retain its highly profitable reputation.

VII. SANITARY ADVANTAGE OF DOING AWAY WITH ILLUMINATING GAS AS A MEANS OF ILLUMINATION.

Any process of illumination which returns to the confined atmosphere we breathe the products of combustion is theoretically open to objection. All methods of illumination up to the present time have depended on some process of rapid combustion, oxygen being withdrawn from the air, an equal bulk of carbonic acid returned to it, and oftentimes a large amount of heat as compared with the amount of light liberated in the process. If now we can illuminate without the subtraction of vital and the addition of mephitic air, and if, however, we can produce an intense light without a corresponding heating of the surrounding atmosphere, we have made two steps of great hygienic value so far as the illumination and ventilation of rooms is concerned.

There is much reason for supposing that this will be soon accomplished in the wholesale introduction of the electric light. By very many roads a crowd of inventors is pushing forward to this end. The rapid destruction of the terminals with the corresponding need of frequent adjustment is being obviated by a variety of devices. In some the length of the carbons is made invariable by a supply of carbon from a hydrocarbon atmosphere in which the electric arc is taken. In another, wasting is prevented by an entire exclusion of oxygen, and the terminals are surrounded by an atmosphere of pure nitrogen. Another experimenter

separates his carbons by an intervening material over which the arc is found, and parts of this electric candle are burnt away at the same rate. In one of these ways, or in some other, the problem of lighting by electricity, more perfectly and as cheaply as by illuminating gas, will be solved, and we shall have the attendant train of hygienic benefits. In this matter of street illumination, the contamination of the atmosphere by gaseous products of combustion is, of course, not a matter of great moment, but in the illumination of those places of public assembly, the church, the theater, the lecture-room, the improvement will be of much importance.

VIII. SANITARY SURVEYS.

The intimate relation between health and the configuration of the soil has been recognized from time immemorial. In truth there is reason to suppose that more practical weight was given to it in ancient than in modern states of society; for while in the former security from enemies and the possible exigencies of a protracted siege made it imperative to select high places capable of good drainage for city sites, the demands of commerce are now best met by towns of the lowest level, frequently at the estuaries of rivers or marshes formed at the confluence of great streams. While the demands of commerce are inevitable, the call upon sanitary science to avert as far as possible attendant evils is not the less urgent. For this reason the rapidly increasing bulk of statistical information upon this subject is a matter of much gratulation. The geological surveys prosecuted by the State governments, and latterly extended to the shores of the Pacific by the munificence of the National authorities, have supplied an admirable foundation. The hydrography of sea-coast and the seaboard estuaries has been executed on a basis so broad and solid that the topography and hypsometry of the whole country can be built upon it. In addition to these we have a number of studies of the relation of topographic and geologic features to one of all the various types of disease.

Even before the inquiry instituted by the medical staff of the Privy Council of Great Britain, the extended research by Dr. Bowditch* demonstrated the intimacy of the relation between wet and retentive soils, and the prevalence of consumption, these conditions of surface-structure being chargeable with a thousand deaths from consumption in Massachusetts alone. Subsequently the fluviate and pond basins of Massachusetts were surveyed and mapped out by Kirkwood.

Staten Island having long lain under the ban of insalubrity, a number of gentlemen, interested in its occupancy and improvement, instituted a sanitary survey of the Island. Dr. Elisha Harris and Mr. Frederic Law Olmstead examining into the more general questions involved, and Prof. Newbury and Trowbridge the structural conditions. The influence of the surface soil, and of the underlying rock, its porosity, its bedding and its joints, upon the local climate, the drainage and the attendant salubrity have served as models for the conduct of similar investigations, in various other portions of the United States. Prof. Cook has been engaged for several successive summers in running a series of levels over much of the densely populated water-sheds of the Hackensack and Passaic, with similar objects, and recently a most minute inquiry has been made into the soil, contour and drainage of Hudson County, by Mr. L. B. Ward, C.E. He has determined how much of its area, which includes the communities of Jersey City and Hoboken, and the smaller towns of Harrison, Kearney, North Bergen, Union, Weehawken and Bayonne, is upland, how much is marsh, what portion is rocky, what occupied by soil of various kinds and where possible, the nature of the substrata, with the population on each tract, and their condition in regard to sewerage and drainage. The ability of each variety of underlying rock, serpentine, sandstone and trap, to carry off surface water is considered, with the corresponding influence upon the surface temperature, dryness and salubrity. When we consider the abrupt changes of habitat over this crowded area, it will be seen that it offers a field peculiarly favorable for the study of the effect of surface condition upon the rates and causes of mortality. Most fortunately the vital statistics of this district have been tabu-

* Consumption in New England, or Locality one of its Chief Causes.

lated with exceptional fullness and accuracy under the superintendence of the President of the Health Board, Dr. L. H. Elder. These statistics have been investigated by M. E. H. Harrison C.E., of Jersey City, with extreme care. He has plotted upon a unking map every case of fatal illness arising from insalubrious environments, each disease being indicated by a distinctive character. Other maps are in course of preparation, showing the relations of surface, contour, and drainage of soil, of rock, of sewerage and relative density of population over the same area. So far as preventable diseases are concerned, their causes will be made so plain that no inscrutable Providence, but wilful ignorance and neglect will be blamable for falling victims to their inexorable operation.

VIII. UPON THE COMPOSITION OF THE ATMOSPHERE.

In conclusion, gentlemen, I hope I may be permitted to say a few words with regard to one topic of sanitary science which for a long time has more particularly interested me, and which at the present time is the subject of especial study—the composition and purity of the atmosphere. As communities grow more dense, and factories multiply, the sources of aerial impurity augment in a rapidly growing ratio. In England, the Government has been forced to appoint an inspector, the celebrated Dr. Angus Smith, who has made thousands of examinations of the air in all parts of the country, and directs Government interference where persons and property are too much imperilled by atmospheric contamination. In Glasgow, a city analyst has been recently appointed with this special duty.

New York is already showing the effect of the sulphurous and nitrous vapors sent out from its myriad chimneys. Recently the U. S. Signal Officer, in his room at the Equitatable Insurance Building, took out from a carefully wrapped package an instrument which he desired to show me, and it was hopelessly corroded by the acid vapors sent out from the tall chimney of the United States Assay Office near by. In Philadelphia there is scarcely a house front which is not disfigured by some stain of magnesia and lime salts, a result in part due to the acid vapors in the atmosphere. And when rains sweep down and carry with them in solution such agents, they are more powerful to corrode metal and even stone surfaces than would at first appear credible. When one of the normal constituents of the atmosphere is wanting—the ozone—it has largely lost its sweetening and disinfecting powers, and there is much reason for believing that the prevalence or severity of certain diseases is intimately connected with the varying amounts of ozone in the atmosphere. Unfortunately there is much difficulty in estimating the percentage of this constituent of the air, and in guarding against the disturbing influences upon our determinations of other bodies possibly present.

To overcome these difficulties experiments have been on foot in the laboratory of the Stevens Institute of Technology for many months past.

Numerous analyses of the atmosphere collected in various parts of the United States, have been made and recorded. They will serve as contributions towards a beginning of a chemical climatology of this country, and might with great profit to the physician, the agriculturist and meteorologist be vastly extended by Government aid in connection with the Signal Service and the Department of Agriculture. We cannot do, unaided, as much in researches of this character as can be done in the laboratories of the Old World by Government assistance, but we can at least labor in the hope that the time is not distant when the importance of research of this kind, even if it does not end in a profitable invention, will be generally understood and generously encouraged.

THE committee of experts appointed by Secretary Schurz to select from the fifteen plans submitted for the remodeling of the portion of the United States Patent Office destroyed by fire has adopted the plan of Mr. Vrydagh, of Terre Haute, Indiana. The plan embraces the addition of an attic story. The upper portion of the building, which has been used as a museum for exhibition of models and curiosities, will be remodeled and made into office rooms, as more are necessary, and the new attic story will be used for a model room.

FURTHER APPLICATIONS IN THE FLOW OF SOLIDS.*

By HENRI TRESCA, President of the Société des Ingénieurs Civils, Paris.

From "Iron."

It has long been known that heat is developed by the forging of a metal; and in some operations connected with the *platinage* of steel, pieces of steel subjected to blows rapidly delivered, may be raised to a dark-red heat. This phenomena does not ordinarily take place, except in working thin sheets; and it will be shown that, in working thicker pieces, the precise situation of the greatest development of heat can be recognized. In a forging operation which the author has had to conduct on a large scale on an alloy of iridium with platinum, a phenomenon occurred incidentally which engrossed his whole attention, bearing intimately as it did on the deformation of solid bodies. He may be permitted to refer to it, though the experiments are not yet completed; and it will be a source of great satisfaction to him to make known the first results of these experiments to an assembly of English engineers before any publication of them elsewhere. On the 8th of June, 1874, the author simply announced the main fact at the Academy of Sciences, that when the bar of platinum, after having been forged, had cooled to a temperature below that of red heat, it happened several times that the blows of the steam-hammer which at the same time made a local depression in the bar and lengthened it, also reheated the bar in the direction of two lines inclined to each other, forming on the sides of the piece the two diagonals of the depressed part; and this reheating was such that the metal was in these lines fully restored to a red heat, so that the form of these luminous zones could be clearly distinguished. These lines of augmented heat remained luminous for some seconds, and presented the appearance of the two limbs of the letter X. Under certain conditions as many as six of these figures produced successively could be counted simultaneously, following one another according as the piece was lifted under the hammer so as to be gradually drawn down for a certain part of its length. The appearance of these luminous traces can be explained beyond all doubt. They were the lines of greatest sliding, and also the zones of the greatest development of heat—a perfectly definite manifestation of the principles of thermodynamics. That the fact had not been observed before was evidently owing to this, that the conditions necessary to be combined at the same moment had not been present under such favorable circumstances. Iridised platinum requires for its deformation a large quantity of work to be expended upon it. The surface takes no scale, and is almost translucid when the metal is brought up to a red heat. The metal is but an indifferent conductor of heat, and its specific heat is low. All these are conditions which are favorable for rendering the phenomena visible in the forging of this metal, whilst it has remained unobserved with all others. Although this explanation was what was to be expected, the author nevertheless proceeded to justify it by experiments of a more direct character, of which some account will now be given; and which constitute the chief motive, and it may be added the chief point of interest in this communication. Given a bar of metal at the ordinary temperature, if, after having coated it with wax or with tallow on two faces, it be subjected to a single blow of the steam-hammer, the wax melts where the depression is produced, and it is observed that the melted wax assumes in certain cases the form of the letter X, as was observed in the case of the platinum bar. In other cases the limbs of the cross are curved, presenting their convex sides to each other. The heat has then been more widely disseminated, and the wax melted over the whole of the interval by which the curves are separated.

The prism, which has this melted outline for base and for height the width of the bar, represents a certain volume and a certain weight; and if it be admitted that the whole piece has been raised to the temperature of the melted wax, the elevation of temperature represents a

* Read before the Institution of Mechanical Engineers.

certain quantity of heat, or, in the ratio of the mechanical equivalent, a certain quantity of internal work which is directly exhibited by the experiment. In comparing this work with the work done by the fall of the hammer, a coefficient of efficiency is obtained which amounts to not less than 70 per cent. This value cannot be taken as final; it depends upon the conductibility of the metal, on the stiffness of the apparatus, and on the clearness of outline of the melted surface. But what the author is desirous to impress upon the meeting is, that here there is a return to the first methods of Mr. Joule, and that the author's investigations of the flow of solids conduct him to certain thermodynamic demonstrations.

The following are the numerical data for some of these experiments, together with the illustrative figures:—

Name of metal.	Work of the ram.	Form of the impression.	Area of wax melted.	Thickness of the forging.	Volume of the corresponding prism.	Corresponding number of heat-units (heating to 50° C).	Equivalent work, at the rate of 435 kgm. per caloric.	Proportion percentage of total work converted into heat.
	kgm.		eq.	cm.	cc.		kgm.	
Iron....	80	Rectangular.	1.45	2.5	3.63	0.1498	63.72	0.796
"	90	"	1.50	2.5	3.75	0.1547	69.79	0.731
"	110	Wide-spreading.	2.20	2.5	5.50	0.2269	96.44	0.877
Copper..	60	Rectangular.	1.75	2.0	3.50	0.1329	56.48	0.942

In the last experiment, taking as melted the area of wax included between the hammer and the crosses, a useful effect of 94 per cent. is obtained.

Stamping.—The object of stamping is to dispose the relative displacements in given directions, in order to pass from the primitive form, supplied direct by the maker, to the definitive form which is desired to be accomplished. From this point of view, the die is a kind of channel designed to facilitate the flow of the material, and to guide in the most suitable direction or directions. When it is required to draw down, by stamping, a square bar of iron, each blow of the hammer causes transverse enlargement as well as elongation; and the useless enlargement is advantageously obviated if it be prevented by the presence of the sides of the canal. If it be well to employ the stamp in simply drawing down a bar, how much more indispensable is it when the variation of form is more complex? The simple idea of flow supplies material for forming a rational judgment on the successive dispositions of the stamps required for the intermediate operations; and also on the adjustment of the sections of rolls, which are but circular stamps or molds, by means of which iron is drawn out. That all these phenomena are but various forms of flow, of which in most cases the circumstances can be anticipated, may be shown by other experiments which will now be described.

The most characteristic of these experiments is, perhaps, the following:—Having completely effaced the reverse in relief of a piece of money, place the flat surface on a sheet of lead, and flatten the second face in the stamping press. The whole relief of this face will be produced on the face which had been reduced to flatness; and the design of this relief will be imprinted on the lead. This effect is explained by the circumstance that each vertical thread or fibre of molecules, being separately compressed in the direction of its length, flows, when struck, with greater facility into the lead than into the other parts of the piece. The saliencies, as reproduced, are less, no doubt, than in the original relief, whilst the more delicate features are partially obliterated, but the general effect is reproduced, and it is apparent that the flow takes place in the direction of the depth, which is also the direction of least resistance. On the reverse of the sheet of lead, which has necessarily been reduced in thickness by the effect of the imprint, the image will be found repeated in a more confused manner, and it may be distinguished by a peculiar tint, which indicates a well-defined geometrical transformation; the lead having flowed in a horizontal direction, as the

only way of escape when its surface was depressed. This amplification or enlargement takes place in the proportion of 22 to 13, when the plate of lead is $\frac{1}{8}$-inch thick.

An entirely different effect is produced when a metal is struck. The blank piece having been placed in the matrix, the portions which are not to be raised in relief by the action of the press are reduced in thickness, for the benefit of the neighboring portions, which are raised; the metal literally flowing in radial directions, from the hollows to the reliefs by which they are surrounded. If the metal has only an engraved face, it may be made up of several blanks of equal thickness superposed. The same mode of distribution of the molecules takes place, and is manifested by successive imprinting at each face, in which the final relief is more or less obliterated. It is so clearly a manifestation of flow that takes place under these conditions, that if the bottom of the matrix be hollowed out at the center, then, the material which converges from the circumference exciting a pressure towards the center, the central portion of the blank is driven towards the orifice, where it forms a very regularly shaped boss; admitting of the transformation of a relief executed on a plane, into a similar relief on a surface which has become very convex or very concave, according as the design pertains to the upper or the lower face of the blank. To an analogous cause the presence of scars sometimes observed on metals highly relieved, is to be attributed; these scars being produced simply by the junction, during the latter strokes of the edges of the bosses which are formed by the earlier strokes. When the medal is relieved on both faces, if it be made up several plates superposed, it is interesting to remark the successive developments and effacements of the images on both sides of the plates; mingling and merging in each other in a singular manner. Rules cannot yet be formulated for the best forms of the grooves of rolls; but it may be accepted that they should be shaped in such a manner as to utilize as far as possible the natural flow of the metal in the direction of the pressures applied to it. It has been shown that, when a bar has to be drawn out, it is best to prevent any enlargement of it laterally, and to facilitate the longitudinal flow; the die should therefore be carefully gauged, short, and opened out in the direction of the length. It has been seen, also, that in stamping a disc, it may be useful to make use of centripetal compression. Each mode of action has thus its own mode of deformation, of which it is necessary to know how to take advantage. The following is a very remarkable instance. Given a disc of lead 4 inches diameter $\frac{1}{8}$-inch thick; if it be pressed in the stamping machine for a diameter of $2\frac{1}{2}$ inches at the center, the thinning of this central portion is only effected by the flow of the material outwards; and this flow is exactly symmetrical, when the centering is perfect. The exterior border is developed in the form of a tulip. By such means, without the employment of a matrix, geometrical forms of a perfectly definite character may be produced, which may be useful in some cases.

This general disposition of material has been long since observed by MM. Piabert and Morin, in the course of their experiments in drawing out blocks of clay. Around the orifice of entry the clay was thrown out in the form of acanthus leaves; and the same develop ment is to be observed in the displacements which take place when projectiles are discharged against armor-plates. The metal displaced by the projectile is driven forward in flakes or strata more or less involved and dislocated, which have, nevertheless, a striking family likeness to the dispositions previously noticed. The geometrical condition of the development in tulip-form of the plate of lead may be very simply explained. The border of the plate, which makes an effort to retain unaltered its diameter and its thickness, continues to be attached to the central portion, the gradual crushing of which throws out rings which are successively thinner and thinner. These rings have, therefore, at at each instant a given thickness, and by their succession they necessarily form a surface of revolution, which is accurately calculable, on the hypothesis, which is perfectly justifiable, that the volume is constant. The conditions of such development may be modified by the employment of casings of various forms,

but attention will be confined to the case of a concentric casing so disposed as to prevent any increase of diameter. Eight discs of lead 1½ inches in diameter having been placed in a cylinder, a piston of 1.20 inches in diameter is placed upon the pile formed by these plates. Since the material can only escape from the compressive action by the annular space comprised between the piston and the cylinder, it ultimately assumes the form of a sort of tumbler, of which the height is extended to the length of the piston, even beyond the length of the cylinder. The thickness of the tumbler, 0.15 inches, would have been more regular if but one disc of lead or of tin had been employed. But the mode of distribution of the layers in the thickness of the tumbler is in itself a useful subject for consideration. The uppermost plate has been developed, almost in one piece, to the upper edge of the tumbler, being connected by a continuous supplementary part, which becomes gradually thinner until it reaches the foot of the tumbler. The other plates are also developed in a parallel direction, supported by the sides of the cylinder, for a length which may be submitted to the same kind of calculation as that of the plates of the concentric jets. It is the same mode of deformation applied, in the present case, to an annular jet; and the complete analogy between the formulas which give expression to their relations is not one of the least remarkable facts in these transformations.

This method has for several years been adopted in industrial operations, under conditions of precision which are truly astonishing, in which a vertical and cylindrical jet, twelve inches high, is manufactured from a sheet of tin perfectly smooth and of uniform thickness. In the finest specimens of that size, the ends of the tube, which are pared after having been struck, do not show any irregularity exceeding 1-12th inch in height, even though the cylindrical envelope has been suppressed for the whole height. The substance driven out in the form of a ring, the thickness of which is measured by the difference between the radius of the punch and that of the matrix, is naturally disposed to form a thin cylinder, the several elements of which slide with equal facility upon the perfectly polished surface of the punch. A thousand examples of similar surprises may be found in industrial processes; but this instance, amongst them all, definitively sanctions the expression by which the author believes he is authorized to designate the results of his researches. The flow of solids is now recognized in science; much more will be accepted by the members, who are witnesses every day of the processes which are based upon it, as the true expression of the best ascertained facts.

Planing.—Of the various operations which have been described above, that of punching is the only one which has had for its object the dividing of a solid body, and forming two entirely separate parts—the burr and the punched block. The block is augmented by compression of a portion of the matter which constituted the cylinder which would have been simply pushed out by the punch, supposing that the cylinder could have slipped out without giving rise to other phenomena. The burr is reduced by the same amount. Cutting or shearing does not really take place until the moment when the burr, in consequence of lateral flow, has been reduced to its final height. It has been proved that from this moment the resistance opposed to shearing is actually proportional to the area of the zone of shearing. The coefficient of resistance applicable to this separation is no other than the coefficient of resistance of fluidity; or what amounts to the same thing, the coefficient of resistance to rupture; so that we are now put in possession of a certain formula, applicable equally to circular shearing by the action of the punch, and to rectilinear shearing by the shear blade or by the turning tool. In each case one of the parts of the piece slides upon the other part, producing at the two sides in contact a drawing out of the successive layers, which are bent over in the direction of the length of the shorn surface, in thin threads, like those produced by the punch. The separation only really takes place at the moment when these shreds are drawn to their extreme limit of tenuity.

This characteristic of the separated surfaces is met with in planing, although

the principal circumstances may here be entirely different; not less remarkable, however. The principal difference consists in this, that the chief compression takes place, not in the solid mass as before, but in the cutting which is detached by the tool, which, as it forms the exterior portion, opposes to the flow the least resistance. If the cutting be compared with the space which it occupied in the block before separation, it is easily observed that it is at the same time considerably shortened, and that, consequently, its thickness has been augmented in the inverse ratio of the shortening. This leading fact in planing is very well exemplified in the turning from the wheel-tire of a locomotive, comprising a cutting from the rivets. These are represented as of an elliptical section, $1\frac{1}{8}$ inch by $\frac{4}{10}$ inch, showing that the reduction in length affected by the action of planing was in the ratio of 10 to 28, or 0.36. This coefficient of reduction is still much greater than it is in many other circumstances; for the thinnest cuttings the coefficient is occasionally as low as 0.10. In another instance, a cutting planed off transversely from a double-headed rail, the height has not been altered, but the width has been reduced nearly in the same proportion as in the first example.

Another characteristic of cuttings produced by planing is, that the surface of the cutting, which rises from contact with the cutting tool, is always smooth and is developed geometrically. That surface, in fact, is moulded on the tool during the process of deformation, and slides upon it in such a manner as to roll itself up in the form of a cone or of a cylinder. At this moment, above all others, the plasticity of the metal is brought into play; and if the original form of the cutting should interpose too serious obstacles to this development, it tears or splits according to the direction of the generating surfaces of contact, still responding to the geometrical condition first referred to. It is well to avoid such rents as much as possible, for evidently they cannot be produced without the expenditure of additional power. Such loss of power must take place, especially where it is required to reduce a curved surface at one cut, of great breadth. An example of such fissures is shown on about a third of the width of another cutting from a tire; but those of the opposite edge are attributable really to a greater reduction of the length of the thinner edge in the process of planing. The other face of the cuttings is always rugged and wrinkled with fissures, or with transverse ridges of very variable aspect, according as the metal is more ductile and the cutting is thicker. For the greater thicknesses both iron and steel present on that surface a multitude of inclined ridges partly covering one another; and of which the incline is still better defined where complete separation has been produced. These scales have been drawn just as they appear under the microscope, on a cutting of Bessemer steel. Nothing can show better than their general inclination the sliding that may be produced in planing, in consequence of the compression which is produced in front of the tool before the cutting is completely detached from the block. In the greater number of cases the turning, when long enough, winds up into a helicoidal form, as may be seen on the cutting, of which the rugged face has just been shown. The inclination of the spirals depends upon that of the cutting edge of the tool, and their diameter upon the thickness of the cutting; the diameter diminishing with the percentage of reduction. It is thus that, in turning in the lathe a piece which is very slightly eccentric, the result is a number of parts of which the diameters are alternately greater and less. The demonstration afforded by this single specimen is quite complete. Without seeking to draw any conclusions from the study of these deformations with respect to the best form of tools for each of them, it follows clearly, from the foregoing discussion, that the work required for any cutting action whatever is expended in friction and in deformation by compression. The work of friction should augment with the number of cuts, and, as the shortening is greater for the finer cuts the molecular work expended should be greater. It follows, therefore, that it is most advantageous to make deep cuts, but, of course this mode of action demands more powerful tools and better foundations. It is in this direction, it appears, that the most recent progress in the manufacture of tools has been effected. The

different modes of cutting, rectilinear or circular, are applicable chiefly to flat surfaces, and to cylindrical surfaces. Flat surfaces are cut in the planing machine or in the lathe, and under most circumstances the two kinds of cuttings are almost identical in appearance—that of a cylinder formed of spirals more or less close, sometimes even in juxtaposition; but for this combination it is necessary that the two edges of the cutting should have been equally reduced, that is, that they should be of the same thickness. If it were otherwise the spirals would become conical; and such of these as appear to be most characteristic will now be described. The cutting obtained in mortising by means of a straight tool is absolutely cylindrical. When the tool cuts out, in this manner, a rectangular groove the material is compressed without any lateral deviation. If the cutting is of great thickness it is triangular, and the smooth surface is formed by the combination of the three faces at which the separation takes place, the direction in which crumpling takes place being the same as in all ordinary cuttings. The triangular form is the result of the compression being greater towards the middle line. To aid in forming an opinion on this point, two blocks were placed side by side, which were planed at the same time in the line of junction of the pieces. Two distant horns were formed, which parted symmetrically from one another; each half-cutting following the law of shortening by which it was bound to assume a form concave towards the side which was held by its attachment to the block. Having made a similar experiment in lead, the parallel and equidistant lines that were drawn upon the block before it was cut could be traced on the cutting, and they afforded the means of measuring exactly the average percentage of reduction and the mode of contortion of these transverse lines, which assumed successively the same inclinations as they lay one upon another at intervals of which the percentage of reduction varied from 0.10 to 0.30. The cuttings from a lathe, when they were produced from the annular groove by means of a straight tool, assumed exactly the same forms. For example, a cutting from a groove in what is called the Swedish piston, is a continuous ribbon rolled up as on a bobbin with the greatest regularity and of great length, without a rent. When turnings take the form of a helix, the small lateral displacement of the piece is not large enough to give to the ribbon a different character to that from a planing machine, when, for instance, it is required to turn a shaft to a uniform diameter, and it is then easy, with good metal, to produce cuttings of great length. But when it is required to turn the end of the shaft or of any cylinder whatever, the cutting follows a special course. If the tool be large in proportion to the diameter of the rings, or circles on which it is acting, the difference of diameter between the two edges of the cutting makes itself felt in the cutting, which assumes the form of a helicoidal surface with inclined generating lines, of which the two directrices are two helices of the same pitch but of different diameters. This universal geometrical character moreover, is manifested in special ways according to the width of the ribbon and the interior diameter of the ring. In this way, three horns may be obtained encased one in the other if the cutting of the tool be radial. Successive spirals foul each other when the direction of the cutting edge is a little inclined. The inner helix is replaced by a straight edge, when the tool cuts right to the center of the face. Notwithstanding these differences of detail, the same rules prevail; a greater or lesser reduction or shortening according to the thickness of the cutting; a less reduction of length at the thicker edge of the cutting; a smooth surface of separation, which always forms a developable surface; a rugged reverse face ridged as if waves of metal had been successively projected there; in fact all the circumstances of a transverse flow of material—setting apart the secondary circumstances of transformation of the prism of metal from which the cutting is produced by augmentation of thickness and corresponding reduction of length. The author endeavored to represent, by a diagram, the triangular cutting which would be formed by planing from the edge of a block of metal a square prism, by means of a tool having two cutting edges, and of which the flat front is itself placed symmetrically. The effect of the

diagram, constructed on the assumption of a percentage of 0.30, is exactly reproduced by the model in relief. In agreement with the foregoing discussion and with the facts, it may be observed how the prism which is on the point of being separated from the block swells up by compression, commencing at a certain zone of fluidity, of limited length, in advance of the tool; and how, when this compression has arrived geometrically at the maximum which could be sustained by the material, the cutting is detached from the mass, to be subjected to the action of the face of the tool, upon which it slides, and which forces it to assume its ultimate form. Considerable as these modifications may appear, they are absolutely in accordance with the facts. They have been produced by the author, on lead as well as on the hard metals, under conditions which were exactly proportional to those which are represented by the model. The finest specimens of this triangular transformation of cuttings that have come under the author's observation, are produced by a mortising tool. They are not less than $\frac{6}{10}$ inch thick, and the rolling up of the metal could only be effected with the accompaniment of deep fissures in the lateral edges. The upper edge, on the contrary, is much more minutely serrated; one of the lateral faces is plaited for its own length—evidence of the compression of the material; whilst the other face, with its oblique fissures, shows still better the sliding by means of which the compression takes effect. There is a still smaller cutting which presents exactly the same characteristics.

It is the author's opinion, that for the construction of the best machine tools, with the most suitable thickness of cuts, the minute examination of the cuttings is of the greatest importance; and that by the same means, the surest evidence may be derived with respect to the qualities and homogeneity of the metal. Time does not permit of more than a passing reference to certain deformations which recall to mind, with a surprising degree of exactness, the constitution of certain rocks with their dislocations. A few experiments of this kind were made by the author in conjunction with M. Daubrée, from which the latter gentleman quite recently derived an explanation of a number of geological phenomena. The results of these inquiries would, no doubt, possess some interest for the members, but the author was desirous chiefly to lay before them such results of his investigations, as followed in natural sequence upon the substance of the communication already made in 1867. The idea of the flow of solids is, of all the modes of regarding their deformation, perhaps the one which most truly interprets all the phenomena of molecular mechanics and of the internal constitution of bodies, which underlie the various industrial operations.

PATENTS AND TRADE MARKS.

By Capt. HENRY GERNER, C. E.

Written for Van Nostrand's Magazine.

It is the purpose of the present paper to give the public an insight into the uses and abuses of protecting that valuable class of the community known as inventors, and also to promote the interests of our merchants in adopting, recording, using and reaping the benefits of trademarks. I may be permitted to speak with some authority on this subject, having had an experience of some thirty-five years in acting as a patent solicitor for myself and others. Nobody will deny that the world has received incalculable benefits from the inventors' fertile brain, and nobody will deny the justice of securing to inventors their inventions, and giving them a monopoly for a certain number of years during which to reap a reward for their ingenuity and toil. How to effect this in a manner satisfactory to the inventor himself and the public at large has been, for all governments, a problem, and is still so to-day. In most of the European countries, anybody who

declares himself to have made an invention, is, upon application, and by due process of routine and practice, presented with Letters Patent without any search or investigation whatever, it being left to the courts to settle all questions arising thereafter and therefrom. Here in the United States it is different. When an inventor applies for a patent, a kind of search is instituted by a corps of examiners who may refuse or give a patent at will. In the majority of cases, a patent is refused on the most frivolous grounds, these becoming a source of much vexation, expense and trouble both to the inventor and his attorney. It is, however, only a trouble which can be easily overcome, like all other troubles in Washington, by aid of the Almighty Dollar! Of course, the officials are unapproachable; you pay your money to somebody on the inside track and you get what you want. I speak from experience. I can name, in my own practice, hundreds of such rejected cases obtained by paying somebody in Washington. Thus, as this system of examination is no guarantee whatever, and only leads to injury, injustice and corruption, it ought no more to be indulged in; and it is to be hoped that Congress, in its wisdom, will abolish this reprehensible system. It is, after all, the courts who will have to pass upon the validity of an inventor's claim; the Patent Office can neither give him anything that he has no right to, nor take away anything that he has a right to. The whole question of all this brain-property, called patents, lies in the question of the *priority* of an invention, and all that an inventor in reality has got, is the unimpeachable proof that he was the first to invent what he claims as his invention. A United States patent, granted to any inventor either by foul or fair means, is no proof whatever that he owns what he claims, and the patent given is not worth the paper it is printed on when anybody else claims and proves that he made the invention prior to the alleged patentee.

Inventors in particular, and the public in general, seem to be entirely ignorant of this fact. The prevailing belief seems to be that a patent granted an inventor is absolute proof that he has a lawful right to all the privileges and benefits the patent purports to give, and it seems to be entirely overlooked that the grant he obtained is only a conditional one. The U. S. Patent Laws, Revised Statutes, of 1870, Section 4886, wisely give an inventor two years' time to put his invention into *public use or on sale* before he is obliged to make application for a patent, or can forfeit his right to a patent. All that is required is that he can produce absolute proof that he was the first to invent what he claims as his, and every inventor should, therefore, as a matter of necessity, have his invention fully and intelligently described and illustrated by drawings in the same way as is customary in patent practice, and such priority of invention documents should be signed and sworn to before a notary public, both by the inventor himself and by witnesses who know that he conceived and perfected the invention on certain fixed dates. This is a legitimate business for patent attorneys, and they should advise inventors asking their aid accordingly, but this not seeming to be profitable enough, inventors are advised to secure a patent in hot haste. On this latter advice, extensive patent mills, both in New York and Washington, flourish. The poor misguided inventor is made to believe and understand that a patent, once allowed, will end all his troubles and open the gates to a Golconda for him. Thus we have to endure the sorry sight of seeing an army of 20,000, mostly abjectly poor, inventors yearly rush in and ask for patents for alleged inventions of which barely one in a hundred are honored by the duplication in any shape of the model left in the Patent Office. Only allowing $100, in the average, as spent by every inventor on these 20,000 alleged inventions, and the patents allowed thereon, it foots up to the round sum of $2,000,000 yearly, wrung from the poor and misguided inventors. If, instead of parting with their own or mostly borrowed money, they had, at a comparatively trifling expense—which might later on be applied to the patent application if advisable—taken pains to properly cover up their priority of invention and then tried to put their invention into "public use or on sale," as the patent laws provide, to ascertain if their invention was marketable, and worthy of the expense of a patent, I believe that everybody who knows anything about the philosophy of patents, will

agree with me that instead of 12,000 patents being issued yearly, not 1,200 would get so far as to be worthy of such a distinction. Patents numbered among these 1200, gone through a fire of an actual trial of merit and actual ownership of invention, would be patents not only in name, as they are now, but patents in fact, and would any day find a regular market and be sought as the best investment of all by capitalists. We would no more see the repeated and pitiful sight under the New York *Herald's* "Business Opportunities," in offers to invest capital, of: "No patent right humbug will be noticed."

Already I fancy hearing the protest of the Patent Office and the patent mills: of the former, because, on the basis of judgment that I have sought to establish, enjoyable sinecures and clerkships will necessarily be dissolved; of the latter because a source of revenue to them, for which no absolutely beneficial return is given, will cease. Let them protest; first comes right, public benefit, national enjoyment; then sinecures and monopolies may follow; I hold that what I have said is true, and I hope to live to see it have become a living fact; it will follow, as a consequence, that the bulk of $398,-024.54 now annually expended for salaries, according to the last annual report of the Commissioner of Patents, will flow into the public treasury. To be sure, it would reduce the number of officials in the magnificent Patent Office to a mere corporal's guard, but then this reduced force would do useful work, by which they would benefit the sovereign people instead of, as now, being a constant source of irritation, injustice, injury and trouble in pretending to do a great deal of work benefiting nobody but themselves, and that in so far as the just quoted salary goes. I hope, for the public's own good, that all these ignorant and pretentious officials may be made to do more useful work in the future than in the past, and learn to behave themselves. I will here not go into the details of my own grievances. I have them in common with most every inventor and patent solicitor. I do not, however, propose to try the suit I have instituted against the managing officials of the Patent Office in the newspapers. If, however, the said officials should provoke an explanation beforehand, I shall be glad to enter into the arena. At least a thousand old clients and others of my friends, in the community wherein I have spent so many years of my life, will know that what I say has its force. I wish for nothing better than that the whole truth might be made known.

Our patent laws, and more particularly their proper execution, need a thorough overhauling. The integrity of the present system was ventilated in Congress last winter, and it is sincerely to be hoped that Congress, during the coming winter, will pass new measures which will prove beneficial alike to the inventor, his attorney, and the public at large: the attorneys especially, among whom are many good and deserving men, at the mercy of unscrupulous and revengeful Patent-office officials. There are several bills now pending before Congress aiming in this direction; among them is that of Mr. Phelps, "To provide for the protection of attorneys doing business before the Patent Office and other bureaus and departments of the Government," presented in the House on May 6 last, read twice, referred to the Committee on Reform in the Civil Service, and ordered to be printed. The penalty it provides for assailing the standing and character of an attorney, or attempting his disbarment, &c., &c., without due process of law, is imprisonment at hard labor and heavy fine.

It should be evident to every thinking man that "an inventor's right to reward and protection exists solely by virtue of the inventor's act of creation." The establishment, by legal means, of this act of creation, should be the basis for a valid patent. If an inventor can establish that he has *priority of invention*, then, and only then, a patent should be granted to him. I would leave it entirely to the courts to settle all other questions, and only require of the Patent Office that it should be an office of record of the inventor's Priority-of-Invention-documents, and the proper executor of patents to such inventors as would claim them. I would have Priority-of-Invention- documents in proper form filed for a registry fee, say, of $5, at the Patent Office; and the first registrar of substantially the same invention should be entitled to a patent after two years had elapsed, not

before and not later. I would require such Priority-of-Invention-documents to be executed in duplicate, and one of them returned to the applicant with a certification of the office thereon that he was the first to register specification and drawing. All later applicants should be denied this certification of being the "first and original"; and it should actually be certified to that somebody has received certification of being the first inventor, much the same as foregoing patents are quoted now in rejected cases by the preliminary examiner. Such certified documents of the first registrar should serve as a "provisional protection," after the style of the British provision, and should as such receive proper recognition and protection from the courts. It should be salable and transferable, being the inventor's absolute property. This will enable the poor struggling inventor to find out if his invention will ever be worthy of a patent; if so, he should have absolute right to a patent at the expiration of two years, but, neither prior to nor later than that expiration. Such a patent would be one of intrinsic value and ought to be paid well for and taxed well. The fee for issuing such a patent which would mostly get into the hands of the capitalists, ought to be $100. I would give the patent a life-time of twenty years, under no circumstances to be extended; and every year a tax of $50 should be required to be paid into the United States Treasury by the patentee or his assignees, under penalty of annulment of the patent.

If this patent system should be adopted, United States patents would rank with United States securities, as the best and the safest in the world. It would give a large revenue to the Government, without any complicated and unsatisfactory work; the public would cease to consider patents a humbug, and the deserving inventor would receive a place in society as a worthy and honored member, instead of being regarded, as he is now, in the light of a bore, a lunatic and a fraud.

I recommend and court a thorough investigation and discussion, by competent gentlemen, of this my proposed plan, and shall be glad, in the best interest of all concerned, to give all the time I can spare, and all the intelligence I can command, to so highly important a subject of so paramount an interest to the welfare and steady progress of this immense and fruitful part of the world in which we have the good fortune to be the pioneers. I believe nothing would prove more beneficial to the United States than a good patent system, and its simple and incorruptible (!) execution. It would stimulate invention to an extraordinary degree, but only the wheat would grow and the chaff perish.

Having thus given my views on patents proper, we now come to a subject of great interest to the trading community. This is the subject of trade-marks, often misnamed patent trade marks, or trade mark patents. It has been well that "the increasing attention now devoted both in this country and in Europe, to the value of property in trade marks, and the very stringent laws that have been enacted both here and abroad, as well as the recent agreement between Great Britain and this country on the subject of trade marks, are among the most significant facts of the times; and open to honest and reputable manufacturers a means of establishing a valuable property right as the result of their labor, skill and patience, which is as tangible as houses, lands, or merchandise itself. It is a sort of crystallization of good will that may be transmitted from generation to generation.

The constitution of a lawful trade mark has been defined by the United States Supreme Court, in Del. & H. Canal Co., *vs.* Clark, as follows:

"A trade mark must be distinctive in its original signification, pointing to the origin of the article; or it must have become such by association; and these are two rules which are not to be overlooked. No one can claim protection for the exclusive use of a trade mark or trade name which would practically give him a monopoly in the sale of any goods other than those produced or made by himself. If he could, the public would be injured rather than protected, for competition would be destroyed. Nor can a generic name, or *a name merely descriptive of an article of trade, of its qualities, ingredients or characteristics* be employed as a legal trade mark, and the exclusive useof it be entitled to legal protection."

It should be known furthermore,—it is curious to note how few are aware of the fact,—that *it is not obligatory to record trade marks in the Patent Office.* This is confirmed in the decision of the Supreme Court of the District of Columbia in J. Rodgers and Sons, *vs.* Philp and Solomons, in these precise words:

"The provisions of the act of Congress of July 8th, 1870, as far as it relates to the subject of trade marks, presents no obstacle to the rights of these complainants to maintain a suit to prevent an infringement of their rights by an imitation of their trade mark, notwithstanding they have ommitted to have their trade mark recorded at the Patent Office. The act is not obligatory on them in this respect. It offers to manufacturers an opportunity to have their trade marks recorded, but imposes no penalty or forfeiture of right for neglect to so record them. Trade marks are the property of their owners independently of statute, and are not the subject of a patent."

Therefore, save your money; save the exorbitant Patent Office registry fee for trade marks. The question of paramount interest to a manufacturer is: whether or not he is in possession of, and is using, a *lawful* trade mark. If not lawful, no Court can sustain it, no matter whether recorded in the Patent Office or not. There are a great number of *unlawful* trade marks recorded in the Patent Office on which the applicant has wasted his money, has got nothing, and fancies himself secured. The several Commissioners of Patents have repeatedly acknowledged this and promised reform. Such promises are idle, as nobody is in want of their fulfillment. Nobody has any need of expending $25 fee now, since no benefit is derived from so doing. The word "*registered*" is believed to be a proof of the legality and ownership of a trade mark; nothing can be more erroneous. For your $25 to the Government you simply obtain a certificate that your own statements about a certain trade mark has been filed in the Patent Office.

It does not guarantee you the absolute right to use the same, nor have you any remedy whatever if the registered trade mark is not a trade mark in the sense of the law. A great number of such I shall be most happy to show conclusively to their owners are not trade marks at all. The patent officials sometimes refuse to register a trade mark after you have paid your $25, alleging that it is not a subject for a legal trade mark. I had many such experiences in my long practice, and almost invariably found that the almighty dollar would make them legal and registrable in the Patent Office. I do not say, nor wish to be so understood, that I paid such money to the officials; I paid it to men who could get what my clients desired, and as it did them no harm, nor any good either, for that matter, but simply satisfied a peculiar whim of theirs and seemed to make them contented and happy, I had no objection, and shall have none in the future, to let such pay for the pleasure of having their trade marks registered in the Patent Office; but my advice always is, was and will be, not to lose money and patience in the attempt to have trade marks registered in the Patent Office. It is like climbing a tree after an apple when just as good a one has droppped from it and lies before you.

Any merchant or trader desiring to adopt or use a trade mark as the means of identification of his goods by the trade and the public, should, in the first place, seek information through proper and reliable sources, of the legality of his trade mark. This satisfactorily ascertained, certain legal declarations, properly verified and *recorded*, are *prima facia* evidence of ownership, which the courts in case of litigation for infringement will, and do, recognize as such.

A GREAT deal of irrigation is effected in Illinois with well water. In Iroquois County, eighty-five miles south of Chicago, 53,500,000 gals. of water from artesian wells are daily, it is said, supplied for irrigating land. No well is over 75 ft. deep. There are 200 wells within a radius of twenty miles, all of small bore. The prairie is 90 ft. above Lake Michigan, and there is no high land for 200 miles which can furnish a fountain head to these wells. A correspondent of the Baltimore *Sun* says that engineers are confident that the subterranean river flowing under San Francisco, leading direct from the exhaustless lakes of the Sierra Nevada, is quite adequate to supply several cities of its size.

MENTAL LOGARITHMS.

By EDW. DAVID HEARN, M.A., Columbia College.

From "The Scientific and Literary Review."

In his introduction to the "Complete and Immaculate Tables of Logarithms," published about half a century ago, Sir Richard Phillips remarked that "Logarithms are the most useful discovery ever made in the principles of arithmetic and calculation, and are so essential to operations in mathematics that in all education the facile use of logarithmic tables should invariably follow the study of decimal fractions." Since that time the teaching of logarithms has become so general that there is now scarcely a schoolboy to be found who is unable to employ logarithmic calculations to abbreviate otherwise tedious processes. To take a very simple example, let it be required to extract the cube root of 45.499593; and we have merely to take either the immaculate or any other reliable table, and turning to the natural number 45.5, which is quite near enough for all ordinary purposes, take out the logarithm, which is 658011, supply the characteristic which, as the integral part of the number, has two digits will be 1, and divide by 3; thus:—

$$3)\ \begin{array}{r} 1.658011 \\ .552670 \end{array}$$

This quotient is the logarithm of the root ought, and again turning to the tables we find that 552668 is the logarithm of 357, and the characteristic being 0 we must write the natural number 3.57; therefore 3.57 is the cube root of 45.499593. Let those who doubt the simplicity and utility of logarithms attempt to extract the required root of the given number by the best known arithmetical process of evolution, and they will find not only that it is complicated and dificult to remember, but that the actual working will take ten times as long as is required to obtain the result by logarithms. To ascertain of what number some other number is the fifth, the seventh, or other high power is often altogether impracticable to the ordinary arithmetician without the use of logarithms; but the example given will suffice for the general reader.

Now, it must be understood that all logarithmic calculation is approximate only, and that it is a very good set of tables that gives the logarithms of natural numbers, as far as 100,000 (that I am using extends to a 10,000 only), but the results obtained are very close to the truth. The larger the natural number for which we can find the logarithm, the greater will be the certainty of accuracy in the results obtained. What then shall we say of a process which enables us to write without the use of tables the logarithm of such a number as 987.654321, and conversely to determine, also without the use of tables, that a given logarithm exactly represents the natural number—

.993020965034979006999?

The processes by which this can be done were explained many years ago by Mr. Oliver Byrne, but probably from his having employed a new system of notation, their application has been extremely limited, and to a large number of young students especially, the entire subject of his treatises has been altogether unintelligible This endeavor will here be made to popularize the system by retaining as far as possible the ordinary Arabic notation.

Some brief references to a few of Byrne's introductory remarks will add to the interest of the present study, and a few words on the manipulation and reduction of decimals will facilitate the accurate comprehension of the subject. Mixed decimal fractions are calculated precisely as whole numbers, all the care required being as to the placing of the decimal point. In addition and subtraction the decimal points must be ranged under each other. In multiplication we must in the product point off as many places from the right hand as there are decimal places in the multiplicand and multiplier combined. And in division we put the decimal point in the quotient as soon as the integral portion of the dividend ceases to be divisible by the integral portion of the divisor. Any decimal fraction can be reduced to a mixed

quantity by using the same multipliers as would be employed in the usual process of reduction, observing the rule of decimal multiplication; and any mixed quantity can be expressed in decimals by dividing step by step as in the reduction process. Thus to show and prove that £3.81875=&3 16s. 4½d., we have merely to employ the following processes:

A.— 3.81875
20
16.37500
12
4.500
4
2.0

B.— 4 | 2
12 | 4.5
20 | 16.375
.81875

Therefore
16s. 4½d. = .81875

In the first working the figure preceding the decimal points represents pounds, shillings, pence, and farthings respectively. In the second working we first write the farthings, and divide by 4, because there are 4 farthing in a penny; then prefix the 4 pence and divide by 12; and, lastly, prefix the 16 shillings and divide by 20: the resulting quotient is the decimal fraction sought. But decimal mixed quantities did not always possess the advantage of the simple notation now used; so that we may hope that something may hereafter be done to simplify Byrne's notation. The first notice of decimals appears to be that found in a tract at the end of Stevinus' *Arithmêtique* in the collection of his works by his friend and pupil, Albert Girard; the tract is entitled *La Disme*. This collection was first published in Flemish, about the year 1590. At this early date decimals in the first palce are termed primes and marked (1); those in the second place are marked (2), and called seconds, and so on; whilst all integers are characterized by the sign (0), which is put after or above the last digit; so that taking an example in addition it would stand thus:

A.D. 1590.	A.D. 1878.
(0)(1)(2)(3)(4)	
3 4 6 1 2	3.4612
2 1 4 7 7 2	21.4772
1 3 0 0 6	1.3006
2 4 0 0 4 9	24.0049
5 0 1 1 3 9	50.1139
(0)(1)(2)(3)(4)	

At the present time most civilized nations not only recognize the importance and simplicity of decimals, but employ the metric system; but the English and Americans are not yet sufficiently enlightened to follow the example, although it may be hoped that hereafter they may become more wise. But all people are naturally averse to change, and hence it is that Arabic figures and notation were not introduced into Europe until about 900 years ago, and were but little used until after A.D. 1600. Leonardo Bonacci, a merchant of Pisa, introduced the Arabian system of digital arithmetic into Italy, and wrote the first treatise published in Europe about A.D. 1228.

In his *Philosophy of Mathematics*, Auguste Comte remarks upon the difficulty experienced in putting mathematical questions into equations, and says that "it is essentially because of the insufficiency of the very small number of analytical elements which we possess that the relation of the concrete to the abstract is usually so difficult to establish. Let us endeavor now to appreciate in a philosophical manner the general process by which the human mind has succeeded in so great a number of important cases in overcoming this fundamental obstacle." He first considers the creation of new functions, and observes that "in looking at this important question from the most general point of view, we are led at once to the conception of one means of facilitating the establishment of the equations of phenomena. Since the principal obstacle in this matter comes from the too small number of our analytical elements, the whole question would seem to be reduced to creating new ones. But this means, though natural, is really illusory; and though it might be useful it is certainly insufficient. In fact, the creation of an elementary abstract function which shall be veritably new, presents in itself the greatest difficulties. There is even something contradictory in such an idea; for a new analytical element would evidently not fullfil its essential and appropriate conditions if we could not immediately determine its value. Now, on the other hand, how are we to determine the value of a new function which is truly simple, that is, which is not formed by a combination of those already known? That appears almost impossible. The introduction into analysis of another elementary abstract function, or rather another couple of functions, for each

would be accompanied by its inverse, supposes then of necessity the simultaneous creation of a new arithmetical operation, which is certainly very difficult."

Let us see how far Oliver Byrne's system meets the requirements referred to by Comte, and how far it can be applied for the development of mental logarithms. Decimal arithmetic, as now taught in schools, has been less than 150 years in use, so that the introduction of mental logarithms can scarcely be objected to on the ground that arithmetical knowledge has already reached perfection. Mr. Byrne explained that because the system of arithmetic invented by him requires numbers to be viewed under two aspects, and to distinguish it from other systems of operating upon numbers, he has called it *dual arithmetic*. By this new art, a number representing any given magnitude, or the function of any given magnitude, may be made to assume a form composed of factors of whole numbers having a known relation to one another; and these derived whole numbers may be readily made to assume a variety of forms, each form always reducible to the given number or magnitude, and hence the derived numbers, by a peculiar arrangement, may be developed to suit different operations; and the factors produced, after such operations are performed, are easily converted into natural numbers, expressing the required results. An acquaintance with the operations of common arithmetic is all that is required to permit of the acquisition of a sufficient knowlege of dual arithmetic for the ready solution of almost innumerable intricate and difficult problems, and in its more complete development it enables the reasoning of the differential and other methods of analysis to be dispensed with. But, as it is not here proposed to treat of the whole subject of dual arithmetic (which cannot be better studied than by consulting Mr. Byrne's own explanations), but merely to show one particular application of it, the theory of the art will only be referred to so far as is necessary to permit of the process being used. All that will be necessary will be to show how the dual logarithm of a natural number can be found, and then how to discover what natural number any given dual logarithm represents; for in the solution of any given problem the dual logarithms (which, as they can readily be discovered without reference to tables, we call mental logarithms) are manipulated in precisely the same way as the common logarithms given in the usual tables.

We are accustomed to represent that imaginary quantity, which is greater than any that can be named by $+\infty$ ("plus infinity"), and that other imaginary quantity which is less than any that can be named by $-\infty$ (minus infinity). The signs will be here used with the same signification. The bases of dual arithmetic are:—

Descending Branch.		Ascending Branch
$-\infty$		$+\infty$
-999999		1000001
-999		1001
-99		101
-9		11
$(1-1=)\ 0$		$2\ (=1+1)$
$.9$		1.1
$.99$		1.01
$.9999$		1.0001
$.99999999$		1.00000001
1	$=$	1

It will be observed that in the case of quantities belonging to the descending branch the power is smaller than its root; for example, the power of 0.9 is only 0.81. In the ascending branch the power is greater than the root. In the descending branch, the bases are greater and greater as they approach 1, but cannot be greater than 1; whilst in the ascending branch the bases are less and less as they approach 1, but cannot be less than 1. Numbers in the dual system of arithmetic are expressed by the continued product of the powers of one or more of the bases, which are seldom introduced into the figurate operations of the art. As, however, the difference between the decimal and the dual systems of notation will probably be best understood from an example we may observe that, according to the decimal or usual system of notation, 73.598 is merely an abbreviated method of expressing:—

$$7(10)+3+\tfrac{5}{10}+\tfrac{9}{100}+\tfrac{8}{1000}$$

which, if we use the commonly accepted method of expressing powers, will become $7(10)^1+3(10)^0+5(10)^{-1}+7(10)^{-2}+8(10)^{-3}$, and we call 1, 0, −1, −2, −3, the powers of the base which, in the decimal system, is always 10. Now, in dual arithmetic it is only the powers of the dual bases which are registered, and each dual digit is the power of a different base; and in order to avoid any departure from the usual Arabic notation we will merely prefix DNA: or DND: to the figures, according as they belong to the ascending or descending branch, and suffix P^1, P^0, and so on, to indicate the power of 10 in the decimal system with which they are connected; thus:—

1.41421356=DNA: 36094110 P.
141421.356=DNA: 36094110 P^5.
.99923682=DND: 00076343 P.

It will be obvious that there is no greater inconvenience in representing a dual number in this manner than in representing an ordinary Brigg's logarithm by prefixing *log.;* and it may here be mentioned that it is proposed to indicate the dual logarithm or mental logarithm by prefixing DL: to the ordinary Arabic numerals in the same way. When some of the dual digits belong to the ascending and some to the descending branch either prefix, DNA: or DND: may be used, care being taken to place a negative sign over the digits belonging to the other branch; thus:

DNA: $35\overline{01}4738P^4$=DND: $3501\overline{47}38P^4$.

For the sake of uniformity it is preferable in such cases always to use the first form. It should be well understood that, although we have given 141421.356= DNA: $36094110P^5$, many dual numbers may be found to represent the same natural number; and it is a peculiarity of the system that, although a given natural number may be represented by a vast number of dual numbers, yet every dual number which represents such given natural number may be almost instantly reduced to one and the same dual logarithm. Dual digits are intimately connected with binominal coefficients, and reference to the arithmetical triangle, formed when the binomial coefficients for the several powers are written down consecutively, will assist the memory for converting dual numbers into natural numbers, and *vice versa*. A line of units is written horizontally, and a second line vertically, the remaining squares being filled by a number equal to those in the squares immediately above and to the left of it; thus:—

1	1*a*	1*b*	1*c*	1	1	1	1	1
1*a*	2*b*	3*c*	4	5	6	7	8	&c.
1*b*	3*c*	6	10	15	21	28	36	&c.
1*c*	4	10	20	35	56	84	120	
1	5	15	35	70	126	210		
1	6	21	56	126	252			
1	7	28	84	210				
1	8	36	120					
1	9	45						
1	10							
1								

Now, we can see at a glance that taking the diagonals consecutively—first, *a*, *a;* then *b*, *b*, *b;* then *c*, *c*, *c*, *c;* and so on—we have the coefficients of the simple quantity, of the square, of the cube, and so on respectively, and these are precisely the operative numbers which we shall hereafter find very useful for giving us our mental logarithms. In the dual system of arithmetic, these numbers are used to find the powers of the bases .9; .999; .99; and so on, as well as the powers of the bases 1.1; 1.01; 1.001; and so on; but it must be remembered that in the descending branch the second, fourth, sixth, and other *even* coefficients must be regarded as negative and used accordingly.

The position of the dual digits is reckoned from the left hand; so that in the quantity DNA:$36094110P^5$, the 3 is said to be in the first position, the 6 in the second, and so on. In the first place, then, we must know how to find the natural number answering to a single digit of either branch in any position. Assuming as before that the working of an example is the readiest means of elucidation, let us find the value of DNA:9 in the first position. The arithmetical triangle gives us, if we read diagonally for 9 either up or down, 1, 9, 36, 84, 126, 126, 84, 36, 9, 1, and we write these in

such a manner that the units shall form one horizontal line, the tens and hundreds running diagonally to the *left* beneath them, thus:—

1	9	6	4	6	6	4	6	9	1
		.	.	.	.	.	.		
	3	8	2	2	8	3			
			.	.					
		1	1						
2	3	5	7	9	4	7	6	9	1

Consequently, as there is nothing to denote that the power of 10 is other than simple, we say that 2.357947691 = DNA : 9 in the first position. When the dual digit stands in the second position, we proceed in the same way, except that we write 0 between each diagonal. Let us, for example, find the natural number equal to the dual digit 5 in the second position; that is, DNA : 05. The arithmetical triangle gives us the coefficients or operative numbers as they are called in dual arithmetic, and we write—

1	0	5	0	0	0	0	0	5	0	1
		.	.	.	.	.	.			
		0	1	0	1	0				
1	0	5	1	0	1	0	0	5	0	1

so that 1.0510100501 = DNA: 05. Upon the same principle, two cyphers, 00, must be inserted between the diagonals when the dual digit is in the third position; three cyphers, 000, when the dual digit is in the fourth position, and so on. We shall presently see that this property of numbers can be very extensively utilized. In arithmetical calculations it is usually sufficient for all practical purposes if we can ensure accuracy to eight places of figures, and we shall observe, if we take any dual digit in the fifth, sixth, seventh, or eighth position, that in the corresponding natural number all the places except one in the decimal fraction portion are filled with cyphers, the exception being the fifth, sixth, seventh, or eighth place, as the case may be, which contains a digit identical with the dual digit. It is unnecessary to show the working, but it will be found that—

DNA : 00002000P = 1.00002000
DNA : 00000070P = 1.00000070
DNA : 00009358P = 1.00009358

The advantage of utilizing this fact is, that we can obtain a dual number correct to eight places of figures corresponding to any given natural number by the calculation of the first four figures only; which calculation can be performed in a few seconds. The natural number corresponding to the last four dual digits is written down, and this is manipulated with the operative number furnished by the arithmetical triangle. The unfound dual digits may be worked off in any order whatever; but care must be taken to locate the results in periods corresponding to the position of the dual digit sought; that is to say, when operating for dual digits in the first position the results must be written down in single figure periods; when operating for dual digits in the second position in two-figure periods; and so on. To take an example, let it be required to reduce DNA: 35014738P⁴ to a natural number. We neglect the decimal and proceed thus:

DNA : 00004738 =	1	0	0	0	0	4	7	3	8	....	1
		3	0	0	0	1	4	2	1	....	3
			3	0	0	0	1	4	2	....	3
				1	0	0	0	0	5	....	1
DNA : 30004738 =	1	3	3	1	0	6	3	0	6		
					1	3	3	1	1	..	−1
DNA : 30014738 =	1	3	3	0	9	2	9	9	5	..	+1
		6	6	5	4	6	4	9	.	.	−5
			1	3	3	0	9	3	.	.	+10
				1	3	3	1	.	.		−10
						7	.	.			+5
DNA : 35014738 =	1	2	6	5	7	0	1	1	5		

operatives for DNA : 3

operatives for DND : 05

∴ DNA : 35014738P⁴ = 12657.0115

It will be observed that the operative numbers are always used as ordinary multipliers, and that all positive products are added, whilst all negative products are subtracted, the final results obtained being the natural number sought. We have seen that DNA: 00004738 = 100004738; and hence, generally, DNA : 0000 $v\,x\,y\,z$ = 1.0000 $v\,x\,y\,z$; when $v\,x\,y\,z$ are digits occupying positions like 4738 in the example. But this is not all, for it will be found that DND: 00004738 is equal to 1.00000000. − 00004738 = .99995262; and generally DND : 0000 $v\,x\,y\,z$ = 100000000 − .0000 $v\,x\,y\,z$; and the knowledge of these facts can frequently be utilized in practice.

The rule for the reduction of common to dual numbers is of course the converse of that which we have just been considering. To find the dual digits for the first, second, third and fourth positions, we first take the common number corre-

sponding to a dual digit of either branch, so that the leading figures of this number may approach the leading figures of the given number; then the dual digits which have to be applied to bring the number selected to the given one will be the other digits of the required dual number. Owing to the properties just referred to, it will be obvious that when the dual digits for the first four positions have been found, the remaining four may, when accuracy to eight places is sufficient, be run in at one operation. By way of example, let it be required to reduce 12657.0115 to a dual number. We will explain the steps of the process after working it.

Given number = 126570115.

DNA : 3	1 3 3 1 0 0 0 0 0	....+1
	6 6 5 5 0 0 0	....−5
	1 3 3 1 0 0	....+10
	1 3 3 1	....−10
DMD : 05	7	....+5
	1 2 6 5 7 6 7 7 6	
DND : 0001	1 2 6 5 7	....−1
	1 2 6 5 6 4 1 1 9	
DNA : 00004	5 0 6 2	4
DNA ; 000007	8 8 6	7
DNA : 0000003	3 8	3
DNA : 00000008	1 0	8
	1 2 6 5 7 0 1 1 5	

We have now only to collect the dual digits and represent the decimal point in the given power by the index of the power, and we have: 12657.0115 = DNA: 35014738P⁴, which proves the correctness of both reductions. With regard to the steps in the process it could be instantly determined by inspection that the common number 133100000 = DNA: 30000000 was the nearest number to 126570115.

As the selected number was too large, we knew that the next dual digit must belong to the descending branch. The number 126576776 was also too large, so that the descending branch had again to be used, and we obtained DNA: 35010000 = 126564119, which is smaller than the given number; therefore, the dual digits in the fifth, sixth, seventh, and eighth position will belong to the ascending branch, and by using such multipliers as will enable us to make up the deficiency we obtain 4738 for the remaining four places.

As there should now be no difficulty in reducing a given natural number to a dual number or in finding the natural number corresponding to a given dual number of either branch, all that remains is to show the method of finding the logarithm of a given dual number; that is to say, the method of calculating a dual logarithm, and then to explain the method of reducing a dual logarithm to a natural number. The first thing to be considered is whether the dual number belongs to the ascending branch, to the descending branch, or is mixed; as the method of reduction differs in each case.

RULE 1.—To reduce a dual number of the ascending branch to a dual logarithm (DL:), regard the given dual number as a natural number, add 31018 times the first dual digit; *plus* 33.09 times the second dual digit; then subtract five times the first three dual digits, a cypher (0) being placed after each, and the remainder, *minus* half the fourth dual digit, will be the dual logarithm sought.

RULE 2.—To reduce a dual number of the descending branch (of eight consecutive dual digits) to a dual logarithm (DL:), regard the given dual number as a natural number, add five times the first three dual digits, supposing a cypher placed after each; 36052 times the first dual digit; and 34 times the second dual digit. The sum will be the dual logarithm sought, and less than one unit out of truth.

RULE 3.—To reduce a dual number consisting of eight consecutive dual digits, some of which belong to the ascending and some to the descending branch, treat those belonging to each branch separately; subtract one result from the other and the difference is the dual logarithm sought; it belongs to that branch which has the greater number.

REMARK.—Dual logarithms are always whole numbers; those of the ascending branch have DLA: prefixed to them; those of the descending branch have DLD: prefixed. It must be remembered that the dual logarithm of 2 is DL: 69314718; and that the dual logarithm of 10 is DL: 230258509, as these logarithms are so frequently required that it is unnecessary to re-calculate them each time.

RULE 4.—To prepare a dual logarithm for reduction to its corresponding dual number, find the difference between the given DL: and some multiple of DL: 230258509, or of DL: 69314718, and repeat the process until the remainder is

less than half of DL: 23à258509, and ultimately less than half of DL: 69314718. When necessary, complete this final remainder to 8 places by prefixing cyphers on the left; then apply one or other of the following rules according as such remainder is a DLA: or a DLD:

RULE 5.—To reduce a prepared dual logarithm of the ascending branch to its corresponding dual number, write down the given DLA: and add thereto 500,000 times the first figure (increased by 1 when the second is 5 or upwards); subtract 31,018 times the first figure from the sum; add 5000 times the second figure of the remainder, subtract 33.09 times the same figure. And, then add 50 times the third figure, *plus* half the fourth figure. The sum will be the dual number sought.

RULE 6.—To reduce a prepared dual logarithm of the descending branch to its corresponding dual number, write down the given DLD: and subtract therefrom 536052 times the first figure; then subtract 5034 times the second figure of the remainder; and lastly subtract 50 times the third figure of the new remainder. The final remainder will be the required dual number.

REMARK.—In applying the fifth and sixth rules the figure multiplied must not alter in the operation, but must reappear in the remainder.

We have placed these rules together, in order to facilitate reference to them, and will now show their application in practice. To economize space we will take one example, which will necessitate the application of all the rules. Let it, therefore, be required to find the DL: of 12657.0115, and prove that the DL: found is the correct one. As we have already found that DNA: $\overline{3}5014738\ P^4$ is a DN: corresponding to the natural number 12657.0115, and since, if our thoughts did not run precisely in the same direction, the resulting DN: would be different we will not recalculate it; but proceed at once to reduce DNA: $\overline{3}5014738\ P^4$ to a DL: which is thus effected:—

Separating the branches in Rule 3, we have DNA: 30004738, and DND: 05010000 to operate upon. We neglect the P^4 for the present and applying Rule 1 to the DNA; and Rule 2 to the DND: the workings will stand thus:—

Firstly.....	30004738
31018×3 (the first dual digit) =	93054
	30097792*
30 00 00×5	=1500000
DNA: 30001738	=DLA: 28597792
Secondly.....	05010000
00 50 00×5	25000
36052×0 (the first dual digit)	0
34×5 (the second dual digit)	170
DND: 05010000	=DLD: 05035170
Lastly..... DLA:	28597792
DLD:	05035170
	DLA: 23562622
P^4=230258509×4	=DLA: 921034036
DNA: 35014738P^4	=DLA: 944596658

Consequently we find that the natural number 12657.0115 is equal to DLA: 944596658, and we have now to prove that this is correct. We first apply Rule 4 to eliminate the powers of 10.

Given DLA:	944596658
230258509×4=P^4=	921034036
Apply Rule 5 to.....	23562622
500000×2 (first dual digit)	× 1000000
	24562622
31018×2 (first fig. of rem.)	− 62036
	24500586
5000×4 (second fig. of rem.)	+ 20000
	24520586
33×4 (second fig. of rem.)	− 132
	24520454
50×5 (third fig. of rem.)	+ 250
Half of fourth fig. of rem.)	1
DLA: 944596658	=DNA: 24520705P^4

The remainder of the proof is simple enough, since it is obvious that we have merely to reduce DNA: 24520705 P^4 to a natural number, by the process with which we are already familiar; thus:

(*See process on following page.*)

Now a very peculiar circumstance may be noticed in connection with this series of reductions—in obtaining the DL: from the natural number we passed by way of one DN: whilst, in calculating the natural number from the DL: we passed by way of an entirely different DN: yet both these dual numbers are alike reducible to the same natural number. Moreover, if we

* The second dual digit being 0, the constant 33.09 is not used.

DNA : 2	=	1 2 1 0 0 0 0 0 0	
		4 8 4 0 0 0 0	
		7 2 6 0 0	DNA . 04
		4 8 4	
		1	
DNA : 24	=	1 2 5 9 1 3 0 8 5	
		6 2 9 5 6 6	
		1 2 6 0	DNA : 005
		1	
DNA : 245	=	1 2 6 5 4 3 9 1 2	
		2 5 3 0 9	DNA : 0002
		1	
DNA : 2452	=	1 2 6 5 6 9 2 2 2	
		8 8 6	(7
		7	(5
DNA : 24520705	=	1 2 6 5 7 0 1 1 5	

$\therefore$ DNA : $24520705 P^4 = 12657.0115$

reduce DNA: 24520705 P^1 to a DL: we shall find that we again obtain DL: 944596658 correct to eight places of figures; which is certainly strong evidence in favor of the accuracy of Byrne's rules, and should suffice to give universal confidence in the importance of the art.

In conclusion it must be understood that it is not proposed to supersede ordinary logarithmic tables by mental logarithms, but to use the dual arithmetical system as an auxiliary to and extension of the usual system of logarithmic calculation; and it will be found when the dual system is once mastered, as it quickly can be with a little attention and perseverence, that a great variety of problems, the solution of which usually involves complicated and laborious processes, can be accurately and readily solved without tables, without mental labor, and by the application of simply and easily remembered rules. For example it can quickly be ascertained that if—

$$3.01416x^5 - 28.233x^4 + 923.7x^3 + \\ + 1234x^2 - 1862x = 1609149128,$$

then x is equal to 56.43657, which would usually require much labor; and the dual system equally facilitates the calculation of angular magnitudes and trigonometrical lines, the solution of plane triangles, without the use of tables, the determination of the numerical value of elliptic and hyperbolic functions, the extraction of the roots of equation of all degrees, and indeed the solution of almost innumerable problems which have hitherto been considered to require great mathematical skill; all this moreover can be done by any one acquainted with the ordinary rules of arithmetic. But inasmuch as the present object is not to demonstrate the value and scope of dual arithmetic, but merely to show by a practical application of it that its advantages may be thoroughly utilized without the use of a special notation, these few references will suffice, and it is hoped that after the examples which have been worked, and the explanations given, this will be fully acknowledged, and that hereafter the system will be far more generally studied by all classes

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CLEVELAND STEEL.

From "Iron."

CAN Cleveland iron be turned into steel, and the steel thus made be now sold at a profit? Speaking broadly of "steel," we mean steel of suitable quality for rails, boiler and ship plates, and the other heavy branches of its manufacture. The cheapest steel now made and used for the above purposes is manufactured fram hæmatite or other ores uncontaminated by the presence of phosphorus. The Bessemer converter is the agent most largely employed for ridding the iron, subsequent to its delivery from the blast-furnace, of the impurities it has gathered up in the operation of its reduction and separation by fusion from the matrix of the ore. But this great cheapener of steel, the Bessemer converter, has been so far regarded as useless in dealing with cheaper irons like those of Cleveland, because it failed to rid such brands of their most objectionable impurity—phosphorus.

The cheapest mode of manufacture has thus far only dealt with the dearest raw materials. When the cheapest mode of manufacture deals equally well with the cheapest raw materials, then the steel

trade will take another giant stride, and settle in new quarters.

When in 1874 Cleveland turned out about 400,000 tons of iron rails, its manufacturers scorned the name of steel, and none of them cared to spend time or money in investigating a point they thought would never affect them. But before then, sundry far-seeing men, not thoroughly contented with anything short of absolute perfection, essayed to solve the riddle, and, commercially speaking, failed, they therefore laid the matter down, and enriched themselves by easier means.

The easier means have now for some time almost failed, and necessity has become the mother of invention.

Cleveland iron always has been deprived of its phosphorus by the agency of oxide of iron. There are some points, however, about the question, which have only lately been completely realized, and one is the precise conditions under which phosphorus has the least affinity for iron and the greatest for oxygen. The determination with certainty of these conditions (in which temperature was for a long time held to play a most important part) appears to indicate that the best substance for purifying Cleveland iron from its phosphorus is the long-used agent oxide of iron, but that it is inconvenient to use this substance also at the same time for the removal of carbon, as has been the practice since the days of Cort.

To make Cleveland iron into steel, speaking roughly, all that is necessary is to reduce to certain very small percentages its impurities, phosphorus, carbon, silicon and sulphur. In the ordinary puddling furnaces, these have been reduced at one operation, as shown below, from forge pig to puddled bar, and after piling and rolling, a rail was produced from this iron, whose composition is also shown. (*See table on following column.*)

	Forge pig.	Puddled bar.	Rail.
Iron	92.74	98.800	99.00
Carbon	3.11	0.050	0.25
Manganese	0.37	0.052	0.05
Silicon	2.00	0.271	0.22
Sulphur	0.25	0.035	0.02
Phosphorus	1.53	0.036	0.35
Oxygen (in cinder)	—	0.405	0.31
Copper	—	0.027	—
	100.00	100.00	100.20

But in a good steel rail these elements must be proportioned within narrow variations as under:—

	Per cent.
Iron	98.49
Carbon	0.32
Silicon	0.05
Sulphur	0.06
Phosphorus	0.08
Manganese	1.00
	100.00

Thus we see that in the Cleveland iron rail the phosphorus and silicon must be reduced and the carbon increased before it assumes a composition similar to the steel rail; and we need scarcely add that, with the same composition, equal strength and like qualities will, in every respect, be obtained.

Two different means are now projected, by one of which Cleveland will doubtless ultimately become as large a seat for the manufacture of steel as it has been for the production of iron. From recent disclosures we shall not be surprised if the great cheapener, the Bessemer converter, is yet brought into requisition to work out this problem, and whilst writing on this matter we are reminded of another new process in which Mr. Bessemer's so original and invaluable apparatus has found a use—no less, in fact, than to form a factor in the probable future manufacture of copper, sulphur and sulphuric acid.

It has been a generally accepted theory recently, amongst metallurgical authorities, that temperature was the condition that most strongly influenced the affinity of the phosphorus for oxygen and iron respectively; but it is now pretty clearly demonstrated that this was a mistake, and that, provided the cinder is sufficiently basic, phosphorus will leave iron and take to the oxygen in oxide of iron at any furnace temperature.

The two different means above-referred to for making steel from phosphoric iron are, therefore, these:—(1) To blow Cleveland iron in a Bessemer converter provided with a non-silicious lining, the phosphorus being removed from the iron during the operation, either by the agency of oxides put into the converter, or by oxide produced by overblowing. (2) To dephosphorize Cleveland iron by

washing with an excess of oxide of iron, and decarbonize the liquid product in a Siemens-Martin furnace.

We see no grounds at present for declaring that either of these processes is likely to prove so much the cheaper as entirely to exclude the other. The chief item of cost in both cases is oxide of iron, and as much of this must be employed in one case as in the other to obtain a like result.

As regards the first plan, how far silicon may be dispensed with when blowing phosphoric iron in the Bessemer converter, is not yet finally determined. If the carbon is eliminated along with the phosphorus, the heat must be got up by the oxidation of some element, and it is well known that no English iron, low in manganese, can be readily blown with much less than two per cent. of silicon. Now, the more silicon present in the phosphoric iron blown in a converter the more basic oxide must be at hand to hold the phosphorus.

On the contrary, pig-iron low in silicon is just the thing for the washing process in which phosphorus is removed and carbon kept in. A high temperature is unnecessary, and the heat due to the oxidation of silicon is not required, therefore, by this process a non-silicious pig-iron may be employed capable of being dephosphorized by a minimum weight of oxides.

Reducing the operation to its simplest form, the molten iron would be taken from the blast-furnace either periodically or continuously into a suitably-constructed refining furnace, either capable of being mechanically revolved, or agitated, or stationary. In whatever way the Cleveland engineers might work this out, the metal would be therein subjected to the washing of liquid oxide of iron. The process is not one involving heat absorption, but, on the contrary, there is a calorific evolution due to the oxidation of silicon and phosphorus to an amount, we have little doubt, quite sufficient to compensate for radiation, especially if the process could be made continuous. Looking broadly at this process, there is nothing about it, except the cost of the oxide of iron, which would appear to involve much expense. Non-silicious linings and a basic cinder, of course, apply in this case as in a converter.

The Bessemer converter has held its place for making steel rails almost to the exclusion of the open-hearth system, and it may be so in Cleveland in the future. But for mild steel, in which the exact percentages of carbon and manganese are of great importance, there is clearly at present a leaning in favor of the Siemens process.

In comparing the cost of these two systems of making steel, the chief item which strikes one is the difference in the wages account; the amount paid in wages on each ton of Siemens steel being about double what is paid per ton of Bessemer steel. The great length of time, viz., nine to eleven hours, which has so far been required to get out a heat of, say, ten tons of Siemens steel, accounts for this difference. But Cleveland iron, dephosphorized by the washing process, may be transferred liquid to the open hearth, and in the absence of silicon decarbonized with three cwt. of ore to the ton, and run out as good steel in from three to four hours. This would make nearly all the difference in wages, not to mention the saving in coals.

The very ingenious combination at work in Belgium, known as "The Forno-Convertisseur Ponsard," bids fair, under like conditions, even to complete such an operation in a still more limited time, for we are assured that this apparatus is now making good steel in less than four hours out of cold pig-iron and rail ends.

Many minds are now concentrated on the problem of making steel in the cheapest possible way, and there is a deep, but, for Cleveland, a tantilizing interest, in awaiting and noting each successive step. Stagnant and wearily as its fortunes now trail on, we cannot but think that from amongst the means we have glanced at, there is even now a radiant glory just arising to regild and illuminate the iron towers of Middlesborough.

SWITZERLAND has gained a considerable accession of valuable and prospectively productive land, by the opening of the canal from Aarberg to the Lake of Bienne, which has been nearly ten years in construction. About 74,000 acres of marsh land are drained by it, and the banks of Lakes Morat, Neufchatel and Bienne are secured from inundations.

THE REGULATION OF RIVERS.

From "The Engineer."

CERTAIN well-meaning individuals have suddenly become alive to the fact that British rivers stand in need of regulation; that is to say, in other words, that the beds of these rivers want straightening and leveling, and that the construction of embankments is required to prevent low-lying districts from being flooded in winter, while sluices, locks, or *barrages*, are indispensable to keep the level of the water up in dry weather. Having made this discovery, the gentlemen in question immediately got up a species of mild agitation, and at the present moment little else is talked of in some circles. The subject has been in one sense discussed very fully, and an earnest request was made at the recent meeting of the British Association for papers on subjects connected with rivers and their management. The request was complied with, and a great many hastily-constructed productions were read, or taken as read, in Dublin. Also a committee was appointed for the purpose of conferring with the Council as to the advisability of urging upon Government to take immediate action to produce unity of control of each of our principal river basins. It is understood, moreover, that not content with what has been done in London, the gentlemen who have taken the bad condition of our rivers so much to heart are about to hold meetings in the provinces to talk about the whole question, and "see what can be done." The object had in view is laudable, and we have nothing to say against it. It would be a pity that so much energy exerted in a good cause should be wasted; and to prevent this as far as lies in our power, we propose here to show that the apostles of river regulation are beginning at the wrong end, and to explain what are the preliminary steps that must be taken before any good whatever can be done; and this is the more necessary because not a few of the leaders of the movement have apparently lost their heads, and regard as "mere matters of detail" questions of the most vital importance.

As far as the memory of man goes back, British and Irish rivers have proved a source of trouble and anxiety to those who have had the misfortune to dwell on or near their banks. The same statement will apply to rivers in France, Germany, India, America—to rivers all over the world, in fact. This circumstance seems not to be generally known, and it has been gravely argued that floods are now much worse in every respect, and do more damage than they used to do. This is only partly true. Good land drainage means the rapid delivery of storm waters into the nearest stream, and inasmuch as Great Britain is now better drained than it ever was before, it is very likely that the floods are, heavier, more sudden in their occurrence, and caused to occur with less rainfall than formerly. Rivers have always made themselves so obtrusive by their bad behavior, that few subjects are better understood by engineers than the laws which govern the formation of floods, and the means which should be taken to prevent them; and it is not too much to say that half a dozen men might be found at this moment in Westminster, who could each design and carry out a system of work which would effectually regulate British rivers. It is quite unnecessary that we should go into any details to prove this just now; yet a very hasty perusal of much that has been recently written, or spoken, on the subject, will suffice to show that the writers and speakers regard the subject as invested with enormous difficulties of a purely engineering character; and very remarkable schemes, to say the least of them, have been proposed for the drainage of flood districts, the construction of *barrages*, and a hundred other things, necessary or not necessary. The answer to all proposals and schemes of this kind is that when the work needed comes to be done, engineers will do it with great ease; and that amateurs will have to stand on one side. The hyetological conditions of Great Britain are sufficiently simple and fairly well understood. We advise those, therefore, who have the will and the energy to agitate for the reform of our rivers, to dismiss from their minds

all schemes for the actual execution of the work. In a word, the time has not yet arrived for the interference of the engineer. When he is wanted he will step on the stage and play his part to perfection, but we are a long way from this part of the drama at present.

The difficulty which must be combated and overcome before our rivers can be regulated, is much more serious than any which engineers will have have to encounter. It consists in reconciling or overruling the contending interests of river-side proprietors. One of the proposals recently made is that what may be termed district water governments should be appointed, each of which should have the sole control and management either of a given stream, or of a given district traversed by several streams. The Thames Conservancy Board affords an example of one of these governments, which, if not accurate, is at least near enough for our purpose. In Holland, local governments of this kind control all work done on the dykes. The idea is no doubt good; indeed it is very difficult to see how any river, such as the Severn for instance, could possibly be regulated unless some special authority was appointed to attend to it. The moment we have come to this point we are face to face with difficulty number one. How is the board of regulation, or conservancy, whichever or whatever it may be called, to be constituted? Who is to appoint it? and from whom can it derive its authority? There can be but one answer. The Board must be empowered by Act of Parliament to raise the necessary funds by taxing those whose property is to be benefited; to carry out the necessary works, exercising in perpetuity the sole control over the stream; to rectify the course not only of main rivers, but of dozens of tributaries; to alter boundaries which have existed from time immemorial, and interfere all round with the rights of property in a way actually without parallel. How is Parliament to be induced to grant such powers? This is difficulty number two. Let us suppose that it was decided to-morrow that the river Severn was to be regulated. The first question asked by those holding property on the stream would be—Who has decided this? In all probability it would turn out that the originators of the movement were a comparatively limited number of individuals whose property was injured by floods; and these gentlemen would at once have to encounter the opposition of all the other people who, living on or near the banks of the stream, would be liable to be taxed for its regulation, but who would benefit in no way by that regulation. The minority would have to go to Parliament for a bill, and it is not too much to say they would not get it. If they were more fortunate and did obtain it, it would be only after the expenditure of an immense sum in Parliamentary costs and fees of all kinds. Amiable theorists maintain that it is the interest of every one living on the banks of a river to contribute funds for its regulation. We need not dispute the soundness of the theory, but it has no practical value in the present connection, because it is not generally held to be true. Indeed, there are individuals who usually display much shrewd common sense, and yet maintain that in a great many instances the value of property destroyed each year by floods would not pay two per cent. on the capital which would have to be expended to prevent them. Arguments like these cannot be dismissed as "mere matters of detail," for until they are answered, and that to some purpose, rivers will not be regulated in Great Britain, and the movement to which we have directed attention will die a natural death.

The first thing to be done by those who advocate the regulation of our rivers is to ascertain whether those most interested are of the same opinion. If they are, the work may perhaps be done; if they are not, the rivers will not be regulated. Let a committee be appointed either by the Society of Arts or the British Association, or some other competent authority. Let this committee begin at the head or the mouth of some important stream liable to floods, and ascertain step by step as they move along the stream, what are the desires of those to whom the river in a sense belongs. Let it be clearly explained to the farmer that certain engineering works will have to be carried out in his riverside fields, and that he will have to pay considerable sums every year for them. In a word, let the whole truth be put before him, and then ask him what his wishes are.

In the same way, let the mill-owners be consulted, and the proprietors of fisheries. In the course of half-a-dozen such trips as this the committee would acquire the information which no one else possesses, and armed with this, they could either recommend the abandonment of the whole idea, or its active prosecution. To use a parliamentary phrase, the sense of the country should be taken in the first instance, and if this is against the interference of Parliament in riparian matters, nothing can be done. We have already indicated the nature of several questions that may be asked. We regret that the list is not full. Suppose that a committee of the Society of Intellectual Hydrologists, let us say, in the course of such a tour in search of information as we have suggested, were to come across a sturdy riparian proprietor who asked them what business it might be of the Intellectual Hydrologists to inquire into the condition of the river of which he was part proprietor, what reply would be given? We really cannot suggest a satisfactory answer. But the last and most important question of all remains to be put. Have those who are so earnest for the regulation of British rivers any money wherewith to even commence to carry out the work they have so much at heart? If they have not, what is to be the end of conferences, and papers and discussions?

We have asked a sufficient number of unpleasant questions. We may be asked in return what our own views are. We can reply in very few words. We have no hesitation in saying that much might be gained by the construction of suitable regulation works on many of our principal rivers; that, as regards others, they are best left alone, because it would not pay to improve them; that any scheme of a comprehensive nature for the regulation or control of rivers must originate with Parliament; and must be a national work, carried out by public funds in the first instance, to be repaid in a certain number of years out of the profits to be derived from the works; that local or isolated efforts, either of individuals or learned societies, are not likely to directly produce regulation work; and that the efforts of such individuals or societies as take an interest in the matter should be devoted to inducing Parliament to appoint a committee to inquire into the whole subject of the arterial drainage of Great Britain and Ireland. In the appointment of such a committee, and in it alone, lies the hope of the would-be regulators of rivers. The interests to be reconciled are too opposed and too great, and the work to be be done is too important to be dealt with by individuals or scientific bodies. Any regulation work which will deal with our great rivers must be undertaken by the nation, and regulation operations which do not deal with our great rivers must be too limited in their scope to possess much value or deserve much attention.

THE STRENGTH OF WROUGHT IRON.—A series of experiments has been carried on at the Washington Navy Yard, by Commander L. A. Beardslee, of the United States Test Board, to ascertain the strength of iron used in chain cables. It had been suspected, with just reason, that the British Admiralty tables for the strength of wrought iron needed revision. Not less than 2000 tests of the tensile strength of iron have been made at the Navy Yard, to determine the elastic limit, elongation, and reduction of area of the various specimens; and 42 complete chemical analyses have also been performed. Some of the conclusions which have been reached are remarkable. The admiralty tables are declared unsafe, and new ones have been prepared. The Board finds that the tenacity of two-inch bar for chain cables should be between 48,000 and 52,000 pounds per square inch; one-inch bar, between 53,000 and 57,000; and that stronger irons than these make inferior cables, because they have less ductility and capacity for welding. The strength of wrought-iron and its welding power are influenced quite as much by the reduction it has undergone in rolling as by ordinary differences in its chemical composition. In general, the processes for making wrought-iron give an uncertain quality to the product, while the methods of making cheap steel confer certainty and uniformity. The ordinary practice of welding is capable of great improvement, by being performed in an atmosphere freed from oxygen. The importance of the subject will be conceded, since the safety of human lives must often depend upon the strength of a ship's cable, or of the links in a bridge chain.

THE STRENGTH OF MATERIALS.

By WILLIAM KENT, M.E., Pittsburgh, Pa.

Written for VAN NOSTRAND'S MAGAZINE.

I.

INTRODUCTION.

The literature upon the subject of Strength of Materials is very extensive. Every professional engineer has, or should have, access to a library of volumes containing records of experiments made for more than a century past, upon every known material of construction, together with mathematical and logical discussions of various theories of strength and resistance, sufficient to enable him to design and proportion structures with that rough approximation to accuracy and economy of material which is at present allowed in most branches of engineering.

In some departments of engineering construction, notably in our American bridge building, the greatest care is taken to thoroughly understand and apply the principles of strength of materials, and to use materials of known quality; so that in both the theory and the practice of bridge construction, the engineers in charge have gone even beyond the books, and have done better work than any that the books have yet recorded.

In the large majority of constructions, however, this care is not taken. In many cases engineers are not employed at all in designing structures, and, in a certain degree, every man is his own engineer. This is especially true in the construction of ordinary buildings. The results are, in most instances, a reckless waste of constructive material, and frequently, a want of correct proportioning; heavy pieces being placed where light ones should be, and *vice versa*. The waste of constructive materials annually in this country might be figured in millions of dollars. On the other hand, the cost of saving material where it should not have been saved has too often been the sacrifice of human life.

It is chiefly for the benefit of those users of materials of construction who are disposed to be their own engineers that this series of articles is written, but it is hoped that they will not on that account be without interest to members of the engineering profession.

It is intended to present some facts and figures which will show that because a metal is called by the name "iron" it does not therefore necessarily possess a definite strength, but that its strength should first be determined by test; that published records of tests are not always to be relied upon; that many tests themselves are not reliable; and that in using any material in construction not only its strength but its other properties should be considered.

The articles will consist principally of compilations from various authorities, American and foreign, to whom credit will be given as far as possible; but many facts and figures will be given obtained from the writer's own experiments, and from those of his friends, which have not heretofore been published.

THE STRENGTH OF MATERIALS.

The term "strength of materials," in its widest sense, as used by many authorities, does not include merely what is known as the absolute or ultimate strength—or the resistance, expressed in pounds per square inch or other unit, to final rupture—but also the resistance within certain limits of distortion short of final rupture, as the elastic limit and the point of permanent set; the safe load; the resistance to steady and to suddenly applied loads; and the resistance to repetitions of loads and to shocks and vibrations. It also includes the amount of distortion of the material before final rupture, or within any limit short of final rupture, commonly called ductility; and the property of returning towards its original form after temporary distortion, or elasticity.

DEFINITION OF TERMS.

The external forces applied to materials tending to cause their rupture or alteration of form are called *stresses*.

They are of different kinds, viz. tensile, compressive, transverse, torsional and shearing stresses.

A *tensile stress*, or *pull*, is a force tending to elongate a piece. A *compressive stress*, or *push*, is a force tending to shorten it. A *transverse stress* tends to bend it. A *torsional stress* tends to twist it. A *shearing stress* tends to force one part of it to slide over the adjacent part.

Tensile, compressive and shearing stresses are called simple stresses. Transverse stress is compounded of tensile and compressive stresses, and torsional of tensile and shearing stresses.

To these five varieties of stresses might be added *tearing stress*, which is either tensile or shearing, but in which the resistance of different portions of the material are brought into play in detail, or one after the other, instead of simultaneously, as in the simple stresses.

TENSILE STRESS.

Testing Machines.—The resistance of materials to tensile stress is the one which receives most attention, as it is called into play more frequently than any other, except compressive, and is considered to be in some measure an index of all the other resistances. It is usually determined by means of an apparatus known as a testing machine. The character of this machine may vary with the nature and strength of the pieces to be tested. To test the tensile strength of a piece of twine, for instance, a convenient apparatus would be a spring balance, the twine being fastened at one end to a firm support, and at the other to the hook of the balance. It might also be tested by fastening one end to a firm overhead support, and attaching to the other end such a vessel as a tin pail, in which shot or sand could be poured till the twine breaks, and then weighing the pail and its contents. For testing large sections of metal, requiring many tons to break them, the testing apparatus may be a machine of great size and strength, requiring a high degree of skill both in its construction and in its manipulation.

Such machines have been built in this country in considerable numbers. One of the best known was built several years ago by the late Major Wade, for the United States Government. It is described in his Reports of Experiments on Metals for Cannon (Phila., 1856.) Copies of this machine, as improved subsequently by Capt. Rodman, are now in use at the Washington Navy Yark and in the U. S. Army Building in New York. One of these machines has been used for several years in the Woolwich Arsenal, in England.

Fairbanks and Co., the well known scale makers, of St. Johnsbury, Vt., and more recently Riehle Brothers, of Philadelphia, have paid considerable attention to the building of testing machines, and machines of their makes are to be found in various iron-making and manufacturing establishments throughout the country. All of these machines weigh the amount of applied stress by means of a combination of levers and scale beams. In the Riehle machine the stress is applied by means of a hydraulic press. In the Wade and the Fairbanks machines the stress is applied through screws, levers and wheel gearing, or some combination of them, the particular combination varying in different machines.

For testing very large specimens, requiring hundreds of tons to break them, hydraulic presses have been used, in which the stress is registered by gauges showing the pressure of the liquid used in the press. Such a machine is in use at the works of the Keystone Bridge Co. in Pittsburgh. A machine has recently been built by Albert H. Emery of Chicopee, Mass., for the use of the United States Board appointed to test Iron, Steel, etc., but it has not yet been erected in position for use on account of the neglect of Congress to make an appropriation for the continuance of the work of the Board. It is designed for a capacity of 800,000 pounds, and is believed by engineers who have inspected it to be the most accurate machine of large capacity ever made.

John L. Gill Jr. of Pittsburgh, has recently built a testing machine of 100,000 pounds capacity, for general work, which, as he claims, remedies some of the defects possessed by all machines of this class which have heretofore been built.

It is frequently supposed that to obtain the tensile strength of a piece of iron, or other material, any testing machine will answer, no matter how roughly built or how badly used, that the specimen may be of any convenient size and shape, and

that any person of ordinary intelligence is capable of making an accurate test. A review of what has been written by various "authorities" will convince any one how erroneous is this idea.

Mr. Kirkaldy, of London, the eminent experimenter, who has written several works containing his observations, quotes Mr. S. Hughes as saying that writers on the strength of materials in the last century seldom assigned a less tensile strength than thirty tons (of 2,240 lbs.) as the weight which would tear asunder a bar of ordinary wrought iron one inch square. Thus Emerson quotes the tensile strength of bar iron at 34 tons; Telford, 29.29 tons; Drewry, 27 tons; while at the present day Templeton gives 25 tons; Beardmore, 26.8 tons; Brown, 25 tons; and Hodgkinson, probably from more careful experiments than any other, 23.817 tons. In reference to these figures Mr. Kirkaldy states that he does not think that there is any satisfactory evidence in the experiments adduced to show that the iron now produced is inferior to that made during the last century. The difference is rather due to experiments having been performed by so many persons, whilst the pieces tested by each were so few—to the different kinds of apparatus employed—to the results having been more carefully recorded by some than by others—to the extreme meagerness of details, and to the complete want, with a few exceptions, of the makers' names or brands—which rendererd futile any attempt at comparison. The various means employed to tear the piece asunder were—applying weights directly to the specimen, single lever and weights, compound lever and weights, combined with a hydraulic press, hydraulic press alone, with either a loaded valve or a gauge to indicate the pressure.

When such wide variations are found in the figures of the strength of bar iron published by those who are supposed to be authorities on the subject, and from whom accurate statements ought to be expected, is it any wonder that figures reported by manufacturers should vary still more, when their tests are frequently made on inaccurate machines, by unskilled operators, and when the results are apt to be influenced by incorrect and irregular methods of test, and by variety of shapes and methods of preparing test specimens, as well as by self interest?

A striking example of the difference in the apparent strength of wrought iron as obtained from different testing machines is given in a paper in the *Metallurgical Review* for September, 1877, by Mr. John J. Williams. In testing the iron for the Point Bridge, Pittsburgh, three machines were used. One is said to have given the tensile strength from three thousand to five thousand pounds per square inch below what it should indicate, and to have "pulled crooked," causing flat specimens to tear at one edge before the iron was strained to the greatest load it would carry. The second machine was supposed to be "nearly correct." The third machine was found on making comparative tests to indicate, on an average, 10,000 pounds higher than the first, "thus proving conclusively that the ultimate strength of iron depends on where the testing is done, and what kind of a machine is used."

Shape of Specimen.—The shape of test specimen has sometimes an important influence upon the record of strength. It is very common to shape test pieces as

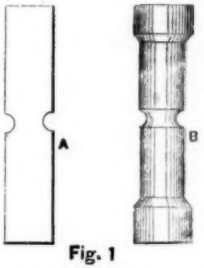


Fig. 1

shown in Fig. 1., in which A represents a piece of plate or flat bar, and B a piece of round iron or other material. In testing pieces of this shape, especially in wrought iron, brass, or other ductile material, the result will almost invariably be higher than the true result as determined by test pieces of the shape shown in Fig. 2. The following facts may be given in confirmation of this statement.

Mr. D. K. Clark, in his "Rules, Tables and Data for Mechanical Engineers" notes a test made by T. E. Vickers, of a piece of steel turned down to one inch in diameter and fourteen inches in length, which gave a tensile strength of sixty

tons per square inch, while a bar of the same steel turned down to $\frac{3}{4}$ inch in a circular notch in the middle, broke at 79½ tons per square inch.

C. B. Richards, C.E., in the Transactions of the American Society of Civil Engineers, Vol. II, p. 339, gives the following results of tests of different-shaped specimens of the same material: For "Burden's best" bar iron the "short" specimens, (shaped as in Fig. 1), gave 62,000 pounds as the average value for the tensile strength per square inch of original cross section of the finished specimen, while the "long" specimens (shaped as in Fig. 2), gave only 49,600

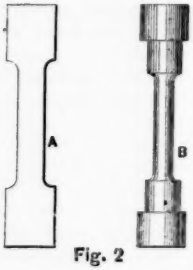


Fig. 2

pounds for that value, the difference in results corresponding to 25 per cent. of the smaller value. For "Bay State" boiler plate, the short specimens gave 52,100 pounds, and the long 47,450 pounds, the variation being 10 per cent. There was a great difference in the ductility of these irons, the Burden bar (long specimens) stretching on an average 30.1 per cent. before breaking, and the Bay State plate only 12.4 per cent., the length of the portion on which the stretch was measured being five inches in each case.

Col. Wilmot found, using proof bars of a form similar to Fig. 3, as a result of

Fig. 3

eight trials of Bessemer steel, a mean of 153,677 pounds per square inch, and on the same steel turned to a cylindrical form three inches long and one square inch in cross section, the mean was only 114,460 pounds per square inch. (Levi and Kunzel's Report on Phosphor-Bronze).

Kirkaldy made some experiments to show the variations in results for tensile strength of Fagersta steel plates, arising from differences in the form and proportions of the specimens. One set was ten inches long and ten inches wide at the parallel middle portion, the second was 1½ inches wide and 4½ inches long, and the third was 2¼ inches wide and 100 inches long. The results were as follows (Clark's Rules, Tables and Data):

UNANNEALED.

	Elastic tensile strength tons per sq. in.	Breaking weight tons per sq. in.	Ratio of elastic strength to breaking weight per cent.	Permanent extension per cent.	Area of fractured section per cent. of original.
Length=breadth	16.05	26.39	60.0	29.7	48.8
Length=3 breadths..	15.56	25.56	60.3	35.0	43.2
Length=44 breadths.	13.71	23.00	59.2	14.0	43.1
ANNEALED.					
Length=breadth	13.53	23.65	57.0	33.1	39.1
Length=3 breadths..	12.98	23.16	56.0	39.3	36.5
Length=44 breadths.	12.04	21.31	56.5	16.5	34.4

Kirkaldy found that the influence of the shape of the specimens upon the results varied with the softness or ductility of the materials, and that this influence was important in the case of soft or ductile materials, but became hardly appreciable with hard and brittle materials.

Richards found that the breaking weight

per square inch of *fractured area* give nearly equal values with either shape. In the case of the "short" specimen the shape tends to prevent any contraction of cross section, and this would tend to make the apparent strength per square inch of fractured section approximate to that of a long piece which contracted to a greater degree.

The writer recently made some tests to determine the influence of shape of specimen upon some samples of charcoal-refined iron furnished by one of the mills in Pittsburgh which has a wide reputation for the superior quality of its product. The following table shows the results:

	Breaking Strength.		Elongation per cent.	Increased strength of short specimen per cent.
	Per square inch of original section.	Per square inch of practical section.		
	lbs.	lbs.		
No. 1.	52,261	97,567	16.	—
No. 2.	54,439	71,668	—	4.2
No. 3.	59,682	76,531	15.	—
No. 4.	63,268	75,601	—	6.0

No. 1 was a piece of flat bar, $2'' \times \frac{1}{2}''$, a "long" specimen, shaped as in Fig. 2, A, the distance between the shoulders being 5 inches and the original section of the portion stretched being $1'' \times \frac{1}{2}''$. No. 2 was a "short" specimen for the same bar, shaped as in Fig. 1 A, the minimum section being the same as No. 1, $1'' \times \frac{1}{2}''$.

No. 3 and No. 4 were a "long" and a "short" specimen from a piece of "C.H. No. 1" boiler plate $\frac{3}{8}$ inch thick, stamped "60,000 lbs.," shaped as in Fig. 2 and Fig. 1 respectively. All the tests were made in the direction of the fibre, all were made on the same testing machine, the same care was taken in fitting, and the same length of time taken in each test, so as to secure as far as possible a uniform set of conditions in each. It will be seen that the short specimen in both cases gave the highest results, although the variation is not so great as that given in the experiments of Richards. The strength per square inch of fractured section of No. 1 is unusually high, and much higher than that of No. 2, the short specimen of the same material. This does not accord with Richards' statement, given above, that the breaking weights per square inch of fractured area give nearly equal values with either shape. The strength per square inch of fractured section of No. 3 and No. 4, however, agree quite closely.

Commander L. A. Beardslee, U. S. N. has recently made a large number of such comparative tests, which confirm the general fact that the short specimens give the highest apparent strength; but the results have not yet been published.

The above results of experiments should be sufficient to show that tests of "short" specimens of ductile materials are of little or no value for giving their actual strength as used in construction. It is a well known fact, however, that in this city and elsewhere, tests are made of short specimens of soft and ductile irons, and the figures obtained published as the "strength" of the materials, when their actual strength as used in a structure is much lower. Such published figures are not only unreliable but unfair. The following reasons may be given why the use of short specimens should be abandoned in tests for commercial purposes.

1. In testing short specimens no accurate measurement can be made of their extension before rupture, as a means of comparison of ductility, a quality, the knowledge of which is quite as important as that of absolute strength.

2. In testing short specimens, two specimens of different materials may give the same results, while long specimens of these materials may give different results. Thus, two short specimens may each show a tensile strength of 60,000 pounds, while if the specimens were of the long shape, one might show 50,000 and the other 55,000 pounds, if one were more ductile than the other. The short specimens, therefore, fail to give not only correct absolute results, but also relative results.

3. There is no *standard* shape of short specimens, and as the difference in results due to various shapes of short specimens is unknown, no proper comparison can be made of the results of different experiments on such specimens.

4. As shown by the experiments given above, there is no standard relation between the strength of a short specimen

and that of a long specimen; hence, if a test is made of a short specimen, it furnishes no means of knowing what strain the material will bear in actual service.

5. As it is always desirable before using any material of construction to know its strength under the most unfavorable conditions, or to know its least, rather than its greatest, strength under any conditions, the test of a sample of the material should show its least strength, and not its greatest, as does the short specimen. Of what advantage would it be to know that a short specimen taken from a bridge-rod will show 50,000 pounds strength, when if a long piece, or the whole rod were tested it would show only 40,000 pounds?

Other Incorrect Tests.—As too high an apparent strength may be obtained in testing specimens of a certain shape, so a specimen of another incorrect shape, or one of a correct shape incorrectly placed in the testing machine, may give too low a result. This occurs when, on account either of the shape of the specimen or of the manner in which it is placed in the testing machine, the line of strain of the machine does not coincide with the axis or central line of the piece tested.

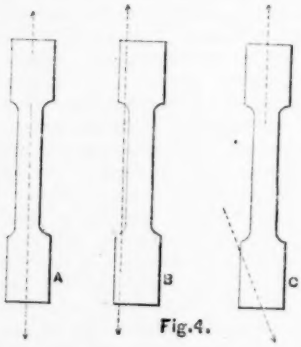


Fig. 4.

In Fig. 4, A is a piece that is incorrectly shaped, the axis of the heads not being in a line with the axis of the middle portion. If placed in the testing machine in the manner that is ordinarily correct, viz.: with the axis of the heads in the line of strain, as shown by the arrows, the test will be an incorrect one and the result will be too low. The piece will break partly by *tearing;* it will *bend* as soon as the strain is applied, and the fibers will give way on one side before the other. B represents a correctly-shaped specimen, but being incorrectly placed in the testing machine, so that the line of strain is not through the axis of the specimen, as indicated by the arrows, it will also break partly by tearing and give too low a result. C is a correctly-shaped specimen, but the upper end is rigidly held in the clamps of the testing machine in the correct position, while from an inaccuracy of adjustment of the lower clamp, or of the testing machine itself, the lower end is pulled sidewise, causing a tearing strain. In all these cases the tendency is to bend the specimen before breaking it. The writer has sometimes seen tests made in which the specimen was so much bent before it was broken that the bend could be detected by the eye, and plainly shown on placing a short straight edge against the side of the specimen. More frequently he has seen tests in which the bend was imperceptible to the eye, but could be detected by an apparatus used for the purpose (which will be hereafter described) which magnified the visible appearance of bending. In many cases, by a slight re-adjustment of the clamps which held the specimen, the bend could be made to take place first on one side of the specimen and then on the other. It may be imagined how valueless the result of a test of cast iron, or of any brittle material would be in which the breaking was caused partly by pulling and partly by bending.

Mr. Hodgkinson states that the strength of a rectangular piece of cast iron drawn along its side is about one-third, or a little more, of its strength, to resist a central strain. It is evident that this ratio (one-third) would be given only by a few certain shapes pulled in a certain manner, and that it would vary with every shape and with every variation of the error of the test. The resistance of any material to tearing bears no relation whatever to its resistance to direct pull, as may be conclusively proven by the simple experiments of pulling and tearing a slip of paper or of tin-foil.

A gentleman in this city states that in making a great number of tests of special grades of cast iron, which have been frequently found by other experimenters to have a strength of about 30,000 pounds

per square inch, he has not been able to find a piece having a strength much over 20,000 pounds. The discrepancy is so large that it can scarcely be accounted for but by supposing that one of the testing machines is not correctly adjusted, or that in the second case the specimens are not correctly placed in the clamps.

The error which may arise in a test of this kind is usually much greater with cast iron or other brittle material than with a ductile material like wrought iron. The ductility of the latter allows it to be partially "drawn into a line," while the former will often break before the drawing into line can take place.

Influence of Time upon Tests.—A difference in results may be obtained in testing two specimens of the same material, if the *time* occupied in one test is longer than that occupied in the other. This difference due to time is in some cases inappreciable, perhaps too small to be measured; but in other cases it is enormous. *With wrought iron and soft rolled steel, the more gradual the test the higher will be the result. With tin, zinc and some other metals and some alloys, the more gradual the test, the lower the result.* These statements are not yet generally believed among unscientific men, probably because it has only been within the last few years that the facts have been brought into prominent notice by experimenters. For abundant confirmation of them, reference may be made to several papers by Prof. R. H. Thurston, published in the Transactions of the American Society of Civil Engineers, in the years 1874, 1875 and 1876. He divides the metals into two classes.

1. "Metals subject to internal strain by artificial manipulation, and which may exhibit an elevation of the elastic limit by strain, and *decreased power of resisting stress under increased rapidity of distortion.* The ordinary irons of commerce are typical of this class."

2. "Metals of an inelastic viscous character, not subject to internal strain, and not usually exhibiting an elevation of the elastic limit by strain, and which offer *increased resistance when the rapidity of distortion is increased.* Tin is a typical example of this class."

Among a great number of experiments by Prof. Thurston—in which the writer had the honor to be associated as assistant—upon the influence of time upon resistance, the following may be mentioned in confirmation of the above statements.

1. Two pieces of ordinary merchant iron, one inch square, taken from the same bar, were tested by bending stress. They were placed on supports twenty two inches apart, and the pressure applied by a screw and registered by a platform scale. One was tested as rapidly as possible, the pressure and deflection being read and recorded at every 20 or 40 pounds, about one hour being occupied in the whole test. A deflection of $5\frac{1}{2}$ inches was caused by a load of 2350 pounds. The other was tested very slowly, and was "rested," frequently under strain, for intervals of from 12 to 48 hours, the whole test requiring more than three weeks' time. A load of 2640 pounds caused a deflection of less than three inches.

2. Two pieces of cast tin, from the same bar, were tested by tensile stress. The test of one was made in eight minutes, and the highest resistance was 3,400 pounds per square inch. The test of the other occupied thirty minutes, and the highest resistance was only 2000 pounds per square inch. Tests by tensile, transverse and torsional stress on tin and on zinc, and on the soft white alloys of tin and copper, invariably gave similar results.

In tests of cast iron, hard steel, and brittle materials in general, the effect of time has not been determined, but it is supposed to be very slight.

Directions for Making Tests.—The object of a test of a material of construction is to learn all that should be known concerning the properties it possesses which make it valuable for the purpose for which it is to be used. No engineer should be satisfied to use in an important structure iron of which he knows only the tensile strength and that inaccurately. He should know its ductility, or amount of extension before breaking, which measures to some extent its resistance to rupture by shock, and its strength within the elastic limit, which measures approximately its resistance to distortion. For many purposes he should know its coefficient of elasticity, or stiffness, its uniformity of strength, and its possession of, or freedom from, internal strain. All

of these may be determined by a tensile test properly conducted, with a correct machine, and an accurate apparatus for measuring elongations. A few directions for making such a test may be of service.

In the first place the testing machine itself should be tested, to determine whether its weighing apparatus is accurate, and whether it is so made and adjusted that in the test of a properly made specimen the line of strain of the testing machine is absolutely in line with the axis of the specimen.

Secondly.—The specimen should be so shaped that it will not give an incorrect record of strength. Under no circumstances should the test of a piece shaped as was shown in Fig. 1 or Fig. 3 ("short" specimen) alone be relied upon to determine strength. The piece should be of uniform minimum section for several inches of its length. The writer recommends five inches in length between the extreme points between which measurements of extension are made, for the *standard* size of specimen, as being the most convenient length for the testing machines now most in use, for calculation of extension in per cent. of length, and for comparison of results with those of other experimenters. Considerable confusion exists in published records of the per cent. of extension of materials as given by different authorities, because they used different lengths in testing. A ductile material may extend 30 per cent. of its length if the specimen tested is five inches long, but if the piece tested is only 1 inch long it might show an extension of 100 per cent. Prof. Thurston gives the following formula for total extension

$$\text{Extension} = Al + fd,$$

where l is the length and d the diameter of the piece, A is a constant, and f is a variable function of the diameter. The length of a specimen, as well as the diameter or area of cross section should always be given in the record of a test.

Thirdly.—Regard must be had to the time occupied in making tests of certain materials. When wrought iron and soft steel can be made to show a higher apparent strength by keeping them under strain for a great length of time, it is well to test them as rapidly as possible to obtain their minimum strength; and in accepting results of tests of these metals from interested parties, it is well to know what length of time they have taken in testing them. It is fortunate that metals of the tin-class are not used in construction to resist heavy stresses. If they were, no test ought to be considered a reliable one which did not occupy a time as long as the material was expected to have "life" in actual service, for, as was shown in the case of the test of tin, above mentioned, the material might be nearly twice as strong under a rapid as under a slow test. In recording the tests of all such materials the time occupied in making each test should be given.

Fourthly—Accurate measures should be made of the extension under each successive increment of load in order to determine all the properties of the material, other than its mere absolute tensile strength, which make it valuable in construction.

Methods of Measuring Elongation.—The method of making measurements of elongation will depend upon the degree of accuracy which is desired. Measurements to $\frac{1}{100}$ of an inch may be made by taking distances by a pair of dividers between two fine lines drawn on the specimen, and reading this distance on a fine metal scale. In testing specimens five inches in length between these lines, this degree of accuracy will give approximately the elastic limit, and the percentage of stretch at different loads beyond the elastic limit, but it will not give the extensions corresponding to loads within the elastic limit, which are a measure of the stiffness, or co-efficient of elasticity, nor the position of the elastic limit with that approach to preciseness which is desirable in competitive tests or in scientific investigations. Reading of extensions to $\frac{1}{1000}$ of an inch may be made by means of two divided plates, clamped one on each end of the specimen and sliding on each other, one carrying a fine scale and the other a vernier; or by means of a micrometer screw, with scale attached, clamped to one end, the point of the screw abutting against a rod clamped to the other end—practically a modification of the micrometer calipers.

For still finer work, readings may be made to $\frac{1}{10{,}000}$ of an inch with accuracy, by the method first adopted at the test-

ing laboratory of the Stevens Institute of Technology, and now used there to the exclusion of all other methods. Two very fine micrometer screws are firmly clamped to one end of the test specimen, the ends of these screws abutting against two rods clamped to the other end. The screws have each fifty threads to the inch, and the head is divided into 200 parts.

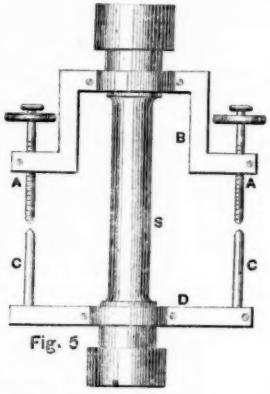


Fig. 5

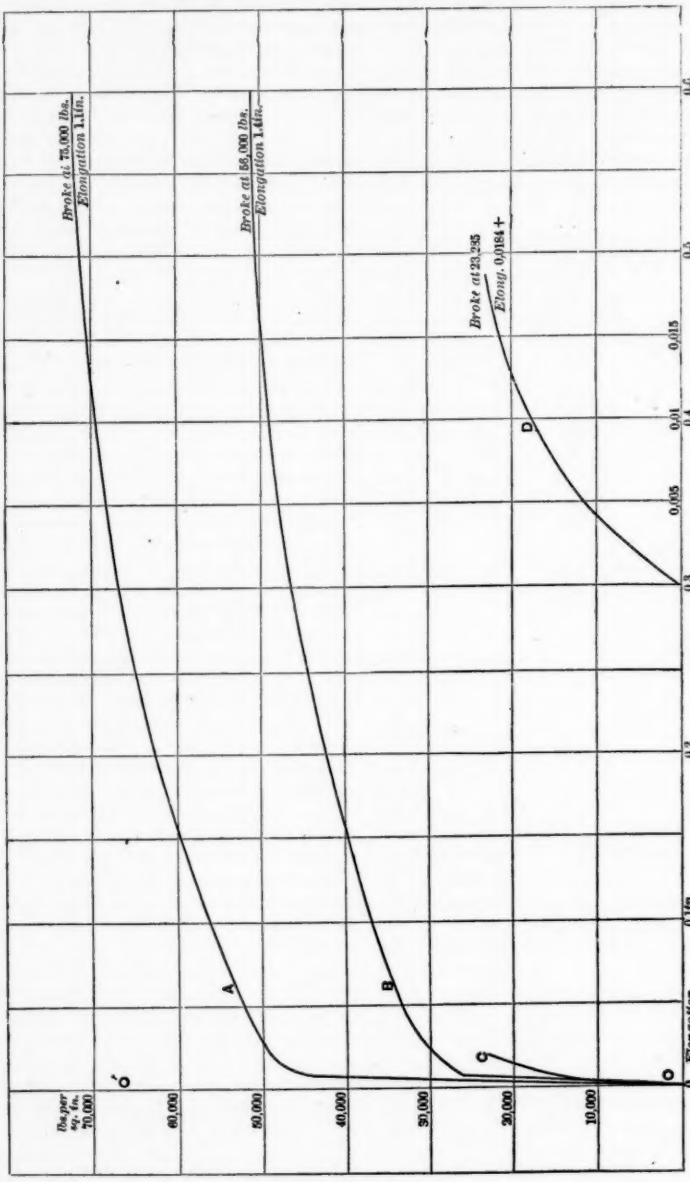
A sketch of this apparatus is shown above. A clamping piece, B, carries the two screws AA, and anotner clamping piece D carries the two rods CC. As the test-specimen S is extended in the testing machine, the points of the screws and rods recede from each other, and the distance each screw requires to be moved forward to make contact at each addition of load is the amount of extension due to such addition. It is impossible to obtain a reading of contact accurate to $\frac{1}{10,000}$ of an inch by the senses of sight or touch, but it is indicated with perfect accuracy by causing the touch of each screw on its opposite rod to close the circuit of a weak electric current and thereby ring a small bell. For this purpose the upper part of the rods CC require to be insulated from the rest of the apparatus. The writer made some experiments to determine the accuracy of the reading by electric contact, and found that there was no error as large as $\frac{1}{40,000}$ of an inch. In taking a reading, the screw A on the right hand is slowly turned till the bell rings, which indicates that contact has taken place. The reading on the divided head is then recorded; contact is broken with the right hand screw and made in like manner with the left hand screw. The difference between the mean of the two readings and the mean of two previous readings is the amount of change of length of the specimen. Two screws are necessary to eliminate the error which would occur if through any cause the piece bends during the pulling. In ordinary tests, there is, perhaps, not one test in a thousand in which the piece will not bend to some extent, and the bending will be plainly indicated by this instrument, although it would remain undiscovered without it.

It is frequently objected that measuring extensions of test pieces to $\frac{1}{10,000}$ of an inch is an unnecessary refinement; but by such measurements only is it possible to obtain the coefficient of elasticity or the elastic limit on specimens only five inches in length of portion stretched, and in many practical cases, as in bridge building, a knowledge of these is desirable. By the method of measurement above described, results have been obtained from five inch specimens of cast iron which are fully as accurate as those obtained by Hodgkinson on rods ten feet long, and with very much less trouble and expense. The following is the record of a test recently made by the writer, of a piece of cast iron, turned $1\frac{1}{8}$ inches in diameter and five inches in length between fillets.

(*See Table on page* 49.)

It will be seen that the greatest difference between the readings indicated by the screw on the right hand and those on the left, is only 0.0026 inches. As these screws were over six inches apart, the amount of bending of the specimen to cause this difference must have been almost infinitesimal. The accuracy of the test is further shown by the great regularity of the increase of the mean elongation and of the decrease of the coefficient of elasticity. The total error of any figure in the column of mean extension is not greater than 0.0002 inch.

Graphic Representation of Results.—The plate on the opposite page is a graphic representation of the results of tests of specimens of cast iron, wrought



iron and steel. The curves, or *strain diagrams* are made by "plotting" the figures of stress and elongation recorded during the test. The test of cast iron represented in the plate is the same as that recorded in the table above. Each curve is a complete record of all the properties of the material which can be

EXTENSION IN FIVE INCHES.

Load.	Right hand.	Left hand.	Mean.	Co-efficient of elasticity
lbs. per sq. in.	inch.	inch.	inch.	
500	0.0007	–0.0005	0.0001	25,000,000
1,000	0.0010	–0.0006	0.0002	25,000,000
1,400	0.0012	–0.0005	0.0003	23,333,333
2,000	0.0014	–0.0003	0.0006	16,666,667
2,500	0.0015	0.0000	0.0008	15,625,000
3,000	0.0017	0.0003	0.0010	15,000,000
4,000	0.0026	0.0000	0.0013	15,384,615
5,000	0.0024	0.0011	0.0018	13,888,889
6,000	0.0027	0.0016	0.0022	13,636,364
7,000	0.0032	0.0019	0.0026	13,076,923
8,000	0.0035	0.0028	0.0032	12,500,000
9,000	0.0039	0.0034	0.0037	12,162,162
10,000	0.0044	0.0038	0.0041	12,195,119
11,000	0.0050	0.0044	0.0047	11,702,128
12,000	0.0053	0.0055	0.0054	11,250,000
13,000	0.0057	0.0061	0.0059	11,016,949
14,000	0.0066	0.0066	0.0066	10,606,061
15,000	0.0074	0.0076	0.0075	10,000,000
16,000	0.0083	0.0086	0.0085	9,411,706
17,000	0.0090	0.0093	0.0092	9,239,180
18,000	0.0097	0.0104	0.0101	8,910,891
19,000	0.0108	0 0116	0.0112	8,482,143
20,000	0.0120	0.0130	0.0125	8,000,000
21,000	0.0134	0.0145	0.0140	7,500,000
22,000	0.0152	0.0168	0.0160	6,875,000
23,000	0.0171	0.0197	0.0184	6,140,218
23,285	broke.			

determined by test. The ordinates, or perpendicular distances of any point of each curve from the base line, represents the applied stress per square inch; and the abscissa, or horizontal distance from the line OO,′ represents the corresponding extension. That point of each curve at which it first bends towards the right hand indicates the elastic limit. The inclination of the initial portion of the curve to the vertical, OO′, measures the stiffness within the elastic limit, or co-efficient of elasticity. This method of representing results is now used by all scientific experimenters on strength of materials.

Elastic Limit and Coefficient of Elasticity.—These terms may here be defined. The elastic limit is that point at which the extensions cease to be proportional to the stresses, and begin to increase in a greater ratio than the extensions. In the diagrams, as stated above, it is the point at which the diagrams begin to curve away from the initial straight line. It will be seen from the diagrams that wrought iron and steel give a well defined elastic limit, while there is no elastic limit shown in the cast iron test, the elongations varying in a more rapidly ratio than the stresses, from the beginning of the test. The elastic limit is sometimes defined as the point at which the first "permanent set" takes place; the permanent set being the extension which remains after the load causing the extension has been removed. Within the elastic limit, according to this definition, a material that is extended by a load will, when the load is removed, return entirely to its original length; and beyond this limit it will only partly return, the amount of permanent increase of length being the set. This definition is not now considered, by the best authorities, as good as the first, as it is found that with some materials a set occurs with any load, no matter how small, and that with others a set which might be called permanent vanishes with lapse of time, and as it is impossible to get the point of first set without removing the whole load after each increase of load, which is frequently inconvenient. The elastic limit defined, however, as the point at which the extensions begin to increase at a higher ratio than the applied stresses, usually corresponds very nearly with the point of first measurable permanent set.

The co-efficient (or *modulus*) of elasticity is a term expressing the relation between the amount of extension or compression of a material and the load producing that extension or compression.

It may be defined as the load per unit of section divided by the extension per unit of length; or, the reciprocal of the fraction expressing the elongation in one inch of length, divided by the pounds per square inch of section producing that elongation.

Let P be the applied load, K the sectional area of the piece, L the length of the part extended, l the amount of the extension, and E the coefficient of elasticity. Then

$\frac{P}{K}$ = the load on a unit of section.

$\frac{l}{L}$ = the elongation of a unit of length.

$$E = \frac{P}{K} \div \frac{l}{L} = \frac{PL}{Kl}.$$

The coefficient of elasticity is sometimes defined as the figure expressing the load

which would be necessary to elongate a piece of one square inch section to double its original length, provided the piece would not break, and the ratio of extension to the force producing it remained constant. This definition follows from the formula above given, thus: If K= one square inch, L and l each = one inch, then E=P.

In the diagrams the coefficient of elasticity within the elastic limit, is indicated by the inclination of the initial portions of the diagram from the vertical, the diagram whose initial portion deviates least from the vertical having the highest coefficient.

Strength Per Square Inch of Fractured Section.—The strength per square inch of fractured section, as determined by dividing the recorded breaking weight by the area measured after fracture, is stated by some writers to be a correct measure of the valuable properties of a material; thus of two samples of iron which show the same strength per square inch of original section, the one which shows the greater strength per square inch of fractured section is the more valuable. In many cases an iron which has a very high tensile strength per square inch of original section is an unsafe iron to use in construction, and one which has a much lower strength is safer, if the latter have the higher strength per square inch of fractured section. Kirkaldy states that for comparing the qualities of iron the breaking weight per square inch of the fractured area should be taken, and *not* the breaking weight per square inch of original section.

This method of comparing the qualities of materials, however, is not as good as that of comparing the strength per square inch of original section *together with the extension before breaking*, and with the uniformity of extension or of reduction of area in different portions of the length of the specimens, for several reasons:

1. The breaking strain per square inch of fractured area in ductile materials is frequently uncertain, as the piece may diminish in section very rapidly in the last few seconds before final rupture, and during those few seconds its resistance decreases rapidly, but apparently gradually, from the maximum recorded resistance of the specimen to perhaps less than half of the maximum. The writer has frequently found that the resistance of a piece recorded an instant before rupture was much less than half the recorded maximum resistance, and so rapidly did this resistance decrease that its final amount could not be measured. In such cases it was impossible to state what really was the resistance per square inch of fractured section. Commander L. A. Beardslee, U. S. N., in his tests at the Washington Navy Yard has found similar results.

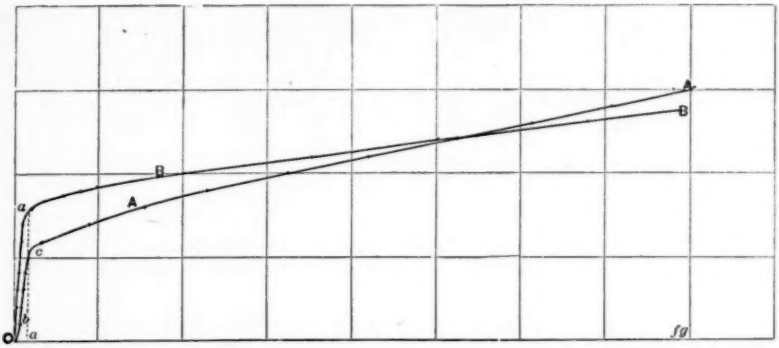
2. In ductile materials the fracture is frequently so much distorted that its area cannot be ascertained with any approach to accuracy.

3. Of two metals of the same class the one which has the greater tensile strength per sqare inch of fractured section *may not* be the better metal. For instance, of two pieces of a certain class of metal, say, soft steel, iron, or brass, of one square inch original section, suppose one has a breaking weight of 60,000 pounds, and the other of 50,000 pounds. The first elongates 20 per cent. and its fractured area is $\frac{3}{4}$ of a square inch. The second elongates 30 per cent., and has a fractured area of $\frac{1}{2}$ square inch. The first would have a strength per square inch of fractured section of 80,000 pounds, and the second of 100,000 pounds, yet it is easily seen that the first is the better metal. It is also seen by this example that the product of tensile strength per square inch of original section, multiplied by the extension, is not necessarily a measure of quality; for in the first case the product is 1,200,000 and in the second 1,500,000; yet the first may be superior, since any elongation greater than 20 per cent. is generally valueless.

4. In tests of metals, or of any materials, having no greater ductility than cast iron, the reduction of section is too small to be conveniently measured, while it is quite easy to measure its elongation. The ductility of such materials therefore cannot be compared by measuring their fractured area, while it can be compared by measuring their elongation with proper apparatus.

True Method of Comparing Qualities of Materials.—The only convenient method of comparing with accuracy all the

qualities of materials, so far as these qualities can be learned from tensile tests, is that of plotting the results of the tests and comparing the "strain diagrams," or the graphic method as above described. Such a comparison may best be illustrated by the two ideal strain diagrams in the cut below. The dots represent the observations made in each test. The specimens are supposed to be of the same shape and size and to be tested in exactly the same manner.



The following record of the two specimens here represented might be given by the operator of the testing machine:

	A	B
1 Tensile strength per square inch of original section....	60,000	54,000
2 Tensile strength per square inch of fractured section..	90,000	80,000
3 Elongation, per cent.....	25	24
4 Elastic limit, lbs. per sq. inch.	22,000	30,000

In a rough test items 1, 2 and 3 only would be given (by some item 1 only) and from these items any one would suppose that A was much the better material. Item 4, if given, might correct this opinion, but it might only lead to doubt as to the accuracy of the test.

An inspection of the curves, however, shows that B is a much better material than A. 1. In B the initial part of the diagram is a straight line to *a*, showing perfect homogeneity and freedom from internal strain; in A the initial line has a bend at *b* showing either want of homogeneity or the presence of internal strain. (This may, however, indicate only an inaccuracy in the record of the first part of the test.) 2. The inclination of the initial portion of B is much less than that of A, showing a higher coefficient of elasticity, usually a valuable property in constructive material. 3. The elastic limit of B is much higher than that of A. 4. The area of the triangle *O a d*, representing the *amount of work* done in extending the piece B within the elastic limit, or its "elastic resilience," is much greater than that of the corresponding triangle *O c d* of the piece A. The piece B, therefore, would be capable of enduring a much greater shock without permanent distortion than the piece A. 5. The total area included between the diagram of B and the base line *O a B B f* is greater than the corresponding area *O c A A g* of the piece A, showing that B has the greater total resilience, or requires a greater amount of *work* to be done on it to produce rupture. Here, then, are five independent points of superiority of the piece B over the piece A, while the bare record of items 1, 2, and 3, given above, would indicate the reverse.

The graphic method of recording results is now adopted by all scientific experimenters. The simplicity and accuracy of the method should commend its use also in ordinary tests for commercial purposes.

With a view to encourage improvements in agriculture in France it is announced that during the year 1879, one gold medal of 1,000f, one silver ditto of 700f., two ditto of 600f., two ditto of 500f., and one of 300f., will be awarded to such landowners or farmers in the Hautes or Basses Alpes as shall have utilized in the most intelligent manner the waters of the irrigating canals.

A PRACTICAL THEORY OF VOUSSOIR ARCHES. PART II.

By WM. CAIN, C.E., Carolina Military Institute, Charlotte, N. C.

Contributed to VAN NOSTRAND'S MAGAZINE.

I.

1. THE following is a continuation of a paper entitled "A Practical Theory of Voussoir Arches," which appeared in this MAGAZINE for October and November, 1874, and afterwards reprinted as No. 12 of *Van Nostrand's Science Series*. In that treatise, the principles affecting the stability of arches, upon the hypothesis of incompressible voussoirs, were exposed and applied to the investigation of numerous experiments upon wooden arches, *at the limit of stability*, with which they were found to agree. A segmental stone viaduct was likewise examined; the theory for incompressible voussoirs being modified *empirically* for the elastic materials used in construction. It was mentioned in the former treatise, that we shall hereafter designate as Part I., that if the effects of the elasticity, causing the deformation of the compressible arch were known, that the investigation of its stability for a statical load could be effected.

The attempt is made, in what follows, to throw some light upon this effect of the compressibility of the voussoirs; and the empiricism, before mentioned, will be criticized in the light of the deductions, as well as from a further consideration of the experiments themselves.

Afterwards, the precise part played by the spandrels will be pointed out and certain theories concerning them discussed. The subject of the theory of arches will then be extended to *underground arches*, *groined* and *cloistered arches*, and *domes;* and practical examples will be given, worked out in detail, to illustrate the investigation—as far as it can be made—of the stability and strength of such structures.

EFFECT OF THE ELASTICITY OF THE MATERIALS.

2. We shall introduce the present subject with some comments on the fourth experiment given in Part I. Fig. 1 represents one-half of a wooden gothic arch and pier of fourteen inches span; the depth of voussoirs being two inches, the horizontal width of pier, 1.9; its height, ten; and the uniform thickness of arch and pier, 3.65 inches. The contour curves of each half arch are described from the opposite springing points. The voussoirs were constructed of equal weight, the pier weighing 2.3 voussoirs. The inner edge of top of pier coincides with the intrados at the springing. With no weight on the crown the arch and pier stood, but fell with a slight jarring, such as a person walking across the room. Now, as the crown joint and joint 5 opened on the intradosal, and joint 3 on the extradosal side, even when the arch stood; the voussoirs bearing at the very edges opposite the opening; it is evident that the arch, at the moment of rupture, was slightly deformed; *i. e.*, did not have exactly the figure above.

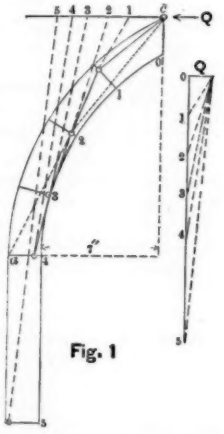
If that deformation had been noted and the figure drawn to correspond, then the resultants of the pressures on joints 1, 3 and 5 would have passed almost through the very edges; but assuming that the arch at the instant before ruptture had the figure above, we find, if the horizontal thrust Q at the crown acts 0″.1 below the summit, that with the value of Q as given, the resultant pressure at joint 3 passes 0″.1 from the intrados, and at joint 5, 0″.2 from the extrados.

To pass a curve of pressure through the points noted on joints 0 and 3, lay off on the direction of Q prolonged, the distances $\overline{c1}$, $\overline{c2}$, ... to the verticals through the centres of gravity of the loads from the crown resting on joints 1, 2, ... respectively. In this case, as detailed in Part I., the distances $\overline{c1}$, $\overline{c2}$, ... are respectively 1.7, 3.19, 4.39, 5.26 and 6.24 inches; the weights resting on joints 1, 2, 3, 4 and 5 being respectively, 1, 2, 3, 4 and 6.3 voussoirs.

These weights are laid off in order on the force line $\overline{05}$ on the right, and a line drawn from 3 parallel to the line 33 in the figure of the arch, cutting off, Q=0.63 voussoirs by scale. Then if from the

points 1, 2, 3 . . . on Q prolonged, we draw lines parallel to the oblique lines through 1, 2, 3, . . . of the force diagram, to the corresponding joints, we find the *centres of pressure* on the joints, the broken line connecting which is called the *line of pressures*. The magnitude and direction of the resultants are given by the oblique lines of the force diagram.

3. Now, if the material of which the arch is composed was absolutely incompressible, *i. e.*, able to sustain any finite effort on a mathematical line, then the centers of pressure at joints 0 and 3 would have passed through the very edges, and the arch would have balanced on a higher pier. We propose to show that this deformation of the arch was due entirely to the compressibility of the material. Again, it is a most important problem to ascertain the distribution of the molecular stresses on any joint, having given their resultant there in position, direction and magnitude. This latter subject is not restricted in its applications to arches alone, but applies to any plane joint or supposed section in a solid on which the resultant of all the external forces acting on the structure can be found; as on any joint of a chimney, retaining wall, abutment, arch, etc.

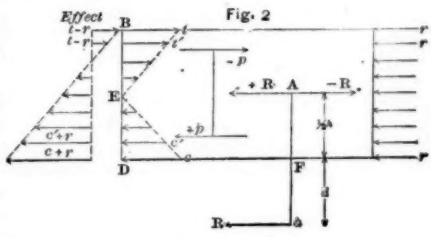


Referring to Fig. 1, we see that the resultants on the joints are not generally perpendicular to them: resolve them into components I. and II. to the joints to which they correspond. The latter, or "shearing force," is resisted by the friction of the joint, resisting sliding, it is only the former component that we shall consider.

4. Conceive a slice DDA, Figs. 2 and 3, of width unity; the cross-section BD, perpendicular to the plane of the paper, being a *rectangle*.

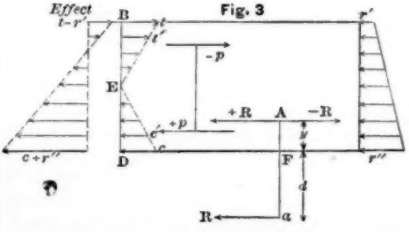
Also suppose the resultant R of all outward forces on the cross-section BD, to lie in a plane parallel to the paper, through the center of the slice and perpendicular to the supposed cross-section. Let us suppose *first* that the cross-section can develop both *tensile and compressive* resistances as in a solid beam, and that R passes through *a*, *without* the cross-section.

If at some point, A in the medial



plane, we conceive two opposed forces +R, −R, parallel and equal to R; the force R with the force −R forms a right-handed couple $\overline{RR}$, that can be replaced by the equal couple $\overline{pp}$ or the forces, *t*, *t'* . . . . *c'*, *c*, equal and opposed to the uniformly increasing tensile *resistances* from E to B and the compressive *resistances* from E to D, E lying in the center of gravity of the cross-section.

5. In Fig. 3 A lies anywhere, in the medial plane, *within* the cross-section (a further limit will presently be indicated); in Fig. 2, A lies in the center of gravity of the cross-section, so that +R passes through E.



The force +R at A, Fig. 2, is decom-

posed into forces, r, r . . . ., *supposed* uniformly distributed. In Fig. 3, +R at A is *supposed* decomposed into r', ..., r'', straight lines, limiting the arrows representing the forces.

Since the final "effect" is to give forces $t-r,'$ . . . . , $c+r,''$ (Fig. 3) limited by *straight* lines, the result must be the same as in Fig. 2, since the forces $t-r$, . . . $c+r$, as limited by straight lines, can have but one disposition in order that R at a may be their resultant.

This would not be so, if some curved line limited the ordinates $r,'$ . . . . r''; which decomposition is thus incorrect if we assume, as is usual in the flexure of beams, that the forces exerted by the fibres and their consequent compressions or extensions, *within the limits of elasticity*, are directly as their distance from the neutral axis, or point of no strain, shown in the "effect" diagram of both figures.

6. PROP.—*If the joint* BD *is a plane joint;* i.e., *can offer compressive, but no tensile resistances, then if* R *falls inside of the joint, anywhere between* A *and* F *Fig.* 2, *it is decomposed into compressive forces only, which decrease regularly from the edge* D, *nearest* R, *towards the other edge, and thus, are proportional to the ordinates of a trapezoid or triangle.*

This is easily proved, if we assume, that *when* one edge of a plane joint is more compressed than the other, the actual shortening of the fibres and hence, (within the limits of elasticity) the forces acting on them, must be directly as their distance from the neutral axis.

There are only three suppositions:

1st. Suppose an *equal* shortening of the fibres on the whole joint. The stresses per square unit are thus the same throughout the whole extent of the joint; but then R, lying on one side of the center, cannot be their resultant. *One edge then must be compressed more than the other.*

2d. But the actual shortening of the fibres cannot be greatest at the edge farthest from R, for then, by hypothesis the stresses must regularly increase in going towards the edge farthest from R; but then R cannot be their resultant. This second supposition is then false.

3d. These two suppositions proving incorrect, the third as stated in the proposition is correct.

R can be, and is, the resultant of the forces distributed according to the law of the trapezoid; the most compressed edge lying nearest R.

7. When R lies so near the edge, that the limit of elasticity is passed in the case of some of the fibers, then although it looks probable that the actual shortening of the fibers is directly as their distance from the neutral axis, yet the corresponding resistances are no longer proportional to the compressions, for those fibers whose limit of elasticity is passed; hence R is no longer decomposed according to the ordinates of a trapezoid, except on a portion of the joint, the resistances being less than by this law as we approach the most compressed edge, for the balance of the joint.

8. Poncelet asserts that the "law of the decomposition of molecular forces at the exterior surface of a solid body" has not been solved hitherto. If we restrain ourselves to practical cases, such as that of one arch stone pressing upon another, or any single block pressing upon another block throughout its whole extent, the above *is* a solution; for in practice we should not allow the resultant to approach so near the edge that the limit of elasticity of any of the fibers is passed; and within this limit the solution is founded upon the same hypothesis as that used in discussing the laws of flexure.

It would seem as useless as impracticable to solve a problem, such as presented by Poncelet, of finding, the distribution of the pressure due to a heavy elastic prism, resting on a non-deformable horizontal plane; or as suggested by another author, of considering the stresses caused by an isolated weight resting on a common pedestal, &c., &c.

9. Where one block rests upon more than one, the decomposition becomes indeterminate. Thus, suppose at the foundation course of a pier, abutment, retaining wall, &c., the resultant passed outside of the middle third. We shall see presently, that if the course resting on the foundation is of one block, then the joint will open at the edge farthest from the resultant.

Where the stones are not cemented together firmly, it is doubtful if the resultant is decomposed according to the law of the trapezoid, where the courses are formed of many stones. The middle third is recommended as a good practical limit however. Still it must not be thought that it is a cure for all evils. Whenever the resultant on any course does not coincide with the center of fig-

ure, there will be settling on the side towards it; so that no pier etc., can be regarded as non-deformable; and the amount of this yielding to allow is simply a practical question; a slight opening of the joints, if not seen, being of itself of no matter, unless the pressures are thereby increased too much for safety, or water is permitted to enter, or some practical objection, other than want of required stability, is experienced.

10. Refering again to Fig. 2, and calling f the strain t or c, we know from the theory of flexure that

Moment of R at a, about A$=$R$(d+\frac{1}{2}h)$
$=\frac{1}{6}fh^2$,

whence,

$$f=\frac{6R(d+\frac{1}{2}h)}{h^2}=t=c \qquad (1)$$

The uniform compression $r=\frac{R}{h}$

$$\therefore(t-r)=\frac{R}{h^2}(6d+2h), \ldots\ldots \qquad (2)$$

$$c+r=\frac{R}{h^2}(6d+4h) \ldots\ldots\ldots \qquad (3)$$

11. Supposing tensile resistances at the joint, these formulæ give correct results for the solid beam; and likewise for a plane joint, when R is so near A, that $t-r$, $t'-r$, . . . , may all act as compressive forces; since, by this decomposition, the law of the trapezoid previously established, art. 6, holds; R being the resultant of the regularly increasing stresses.

12. Now let $t-r=0$. From eq. 2, $d=-\frac{1}{3}h$ or aA$=\frac{1}{6}h$. As long as aA is less than $\frac{1}{6}h$, there are only compressive resistances on the joint; but if the resultant leaves "the middle third" of the joint BD, then $(t-r)$ is positive and we *assume tensile resistances* at B to oppose the forces.

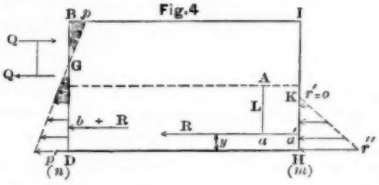
13. *But* suppose the joint BD can offer no tensile resistances, then it is wrong to decompose R as before, if by another method of decomposition stability is assumed. Many writers seemed to have overlooked the method of decomposition proved in art. 6.

Thus, R at a, Fig. 4, is the resultant in position on the joint BD. We may decompose R according to the ordinates of a triangle as at (m), if we assume the law of the trapezoid as before, r' is now 0, hence $y=\frac{k}{3}$, where $k=$KH$=$height of trapezoid or triangle.

Placing

$$\overline{Aa}=L,\ HK=h=3\ \overline{Ha'}=3(\tfrac{1}{2}h-L) \quad . \quad (4)$$

Fig. 4



14. Let us now compare this result with the case shown at (n), where the beam is supposed to supply tensile resistances along BG. Call the resistances per square unit at B and D, p and p' respectively; *i.e.*, $p=t-r$ and $p'=c+r$.

From similar triangles, calling DG$=x$,

$$p' : x : : p : h-x \therefore x=\frac{p'h}{p+p'}$$

Now, replacing $(d+\frac{1}{2}h)$ in eq. (1) by L; deducing p and p' as in (2) and (3), and substituting, we have,

$$x=DG=\frac{\frac{R}{h}\left(\frac{6L}{h}+1\right)h}{\frac{R}{h}\,2\frac{6L}{h}}=\frac{h^2}{12L}+\frac{h}{2} \quad . \quad (5)$$

Now from eqs. (4) and (5)

For L$=\frac{1}{6}h$,	HK$=h$,	DG$=h$,
$\frac{1}{4}h$,	$\frac{1}{2}h$,	$\frac{3}{4}h$,
$\frac{5}{12}h$,	$\frac{1}{4}h$,	$\frac{7}{10}h$,
$\frac{1}{2}h$,	0,	$\frac{2}{3}h$,

That is HC$<$DG, when L lies between $\frac{1}{6}h$ to $\frac{1}{2}h$; or the point of no strain in the beam lies nearer the edge DH, where the joint can oppose no tensile resistances than when we suppose them exerted.

15. Let us now compare the strains p and r''. Representing R by the area of the triangle whose base is KH and altitude r'', we have,

$$R=\frac{kr''}{2}=\frac{3yr''}{2}$$

$$\therefore r''=\frac{2}{3}\frac{R}{y} \quad . \quad . \quad . \quad . \quad (6)$$

Again moment of R at a about A$=$R$(\frac{1}{2}h-y)$, &c. See art. 10. Hence,

$$p'=(c+r)=\frac{R}{h^2}(4h-6y) \quad . \quad . \quad (7)$$

Thus for any values of y between 0 and $\frac{1}{3}h$ (see table) we find that, $r''>p'$, always:

y	r''	p'
$1\frac{1}{2}h$	8	$3\frac{1}{2}$
$\frac{3}{4}h$	4	3
$\frac{1}{2}h$	$\frac{8}{3}$	$\frac{5}{2}$
$\frac{1}{3}h$	2	2
	$\frac{R}{h}$	$\frac{R}{h}$

There is therefore greater compression at edge DH when the real forces are as at (m) than when the beam can oppose tensile resistances as at (n). Also since KH$<$DG, within the same limit ($y=o$ and $\frac{1}{3}h$) the compression is more uniformly distributed at (n) than at (m); hence the beam will bend more when, as in the voussoir arch, a joint BD can only oppose compressive resistances, than when, as in a solid beam, tensile resistances can be exerted. As a consequence of this compression at lower edge, there being none (according to our hypothesis—the law of the trapezoid) at K in case (m), the joint above K must open, and could in fact be removed without interfering with the distribution of the forces at all. . . . Some writers have erroneously asserted that the opening was due to tensile forces. It may be observed, that if, as in the voussoir arch, these tensile forces are unbalanced rupture must ensue.

16. *The decomposition of* R *cannot be the same for an open joint as for a solid beam, when* L *lies between* $\frac{1}{3}h$ *and* $\frac{2}{3}h$; for if, for a solid beam, we assume the disposition (m), the edge nearest R is most compressed, and the joint above K would tend to open; but as there are tensile resistances there that prevent it, the disposition (m) is not correct for the solid beam; hence some other disposition as (n) is correct.

17. It is evident, from the reasoning in art. 6 that, *in a voussoir arch, whenever the resultant on a joint does not pass through its center, that the edge nearest the resultant is most compressed, and the arch is consequently deformed.*

If some external force, as a spandrel thrust, keeps the line of pressures in the middle of the arch ring, then there will be no deformation of the arch, save that due to the uniform compression at each joint.

Conversely, if the arch retains its shape, save that due to a uniform compression at each joint, the line of pressures must coincide with the center line of the arch ring; for otherwise there would necessarily be deformation of the arch, which is not supposed.

18. It is evident, that as the resultant pressures on the joints are farther removed from the center, the deformation is greater; increasing gradually until, when the line of pressures leaves the middle third of the arch ring, the joint just begins to open, this opening increasing, [see Fig. 4 (m)] until when the resultant is very near the edge, the strain r'' on the most compressed fiber exceeds the ultimate strength of the material and crushing ensues, followed or accompanied by rotation. For very light arches this crushing may not be perceptible. Thus, in the experiment given Fig. 1, there was deformation of the arch, before rotating began, due to the compressibility of the material, joints 0, 1 and 5 compressing most at their outer, joints 2, 3 and 4 at their inner edges. The resultants at joints 0, 3 and 5, obeying the law of nature's economy of force, pass nearly through the very edges; so that $k=\overline{KH}=3y$ (Fig. 4, m) is very small, and as the face of one voussoir can be supposed to rotate about K, the opening at I must be very appreciable, as it was at joints 0, 3 and 5 in the actual experiment.

19. It is likewise evident that the greater the compressibility of the material, the greater the deformation of the arch; hence, with weak materials, rotation will occur sooner, *i.e.*, with less loads or lower piers, than for less compressible materials. This principle is proved experimentally by exps. 13 and 14 of part 1, with cloth joints.

The above theory now perfectly explains, that apparent anamoly in the experiments, of the resultant on the base of a pier, made up of several bricks, approaching the center as the height of the pier was increased; the arch and pier being at the limit of stability. The true explanation is, that it is due to a compression of the edges, causing a deformation of the arch; so that if the exact figure of the arch at the instant of rotation could be obtained, we should find that the line of pressures approaches closely the very edges at the joints of rupture.

In fact this is necessarily so, for as these edges alone bore, just before rotating, the line of pressures must pass through them. The same remarks apply to all the experiments. It was not attempted to find the figure of the deformed arch just before rotating, for the figure in most cases was not constant and hence impossible to obtain. In truth a very limited time scarcely permitted of the experiments that were performed.

20. It is well to note that so long as R

remains within the *inner third* of the arch ring, that compressive forces act on the *whole extent* of the joint, and thus there will be no opening. Eq. (3) will give the unit strain on the fiber at the most compressed edge, noting that d is now minus. When $d=-\frac{1}{3}h$, $(c+r)=\frac{2R}{h}$, or double the compression $\frac{R}{h}$, if the resultant were uniformly distributed on the joint.

Hence at those joints where the resultant cuts the joint $\frac{1}{3}$ the depth of joint, from either edge the strain induced in the most compressed fiber, is double that due to a uniform distribution of the resultant on the joint.

21. *It is clear likewise that inversely if in any voussoir arch the joints are all closed, that the line of pressures keeps somewhere within the middle third;* for if it did not, then there would be compression, uniformly increasing, as at Fig. 4 (m) over *only a part* of the joint; so that while the fiber at K is unaltered, those fibers on the part KH are compressed, which necessitates the opening of the joint above K; but this is against the supposition of closed joints, hence the actual line of pressures keeps within the middle third. Eq. (6) gives the value, $r''=\frac{2}{3}\frac{R}{y}$, of the unit strain on the edge nearest R. When R lies in the *outer* third of the arch ring, the mortar being supposed to offer no appreciable tensile resistance.

Thus if R passes $\frac{1}{4}$ depth joint from an edge, $r'=\frac{2}{3}\frac{4R}{h}=\frac{8}{3}\frac{R}{h}$, being slightly over double the unit strain $\frac{R}{h}$, if R was uniformly distributed.

22. For arches of medium spans, it can matter little, so far as strength and stability are concerned, whether, by the aid of a spandrel thrust or other device, or from inherent strength, the line of pressures is restrained to the middle third or to limits, $\frac{1}{4}$ depth joint from edges, or other limits. Thus in the stone viaduct of 50 feet span, given in Part I, the horizontal thrust at the crown, in a slice 1 foot wide, is nearly 25 tons, the depth of voussoirs being 2.5 feet. Now if the compression were uniform, the strain per square foot would be 10 tons.

If the resultant at the crown passed, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$ depth joint from extrados or intrados, the strains per square foot at the most compressed edge would be 20, 26.7, and 33.3 tons respectively. Since the crushing weight of granite varies from 400 to 800, of limestone from 250 to 600, and of sandstone from 200 to 300 tons per square foot, it is evident there can be no danger of crushing in these cases for such materials. The crushing weight of best brick in cement, Trautwine gives as 50 to 70 tons per square foot; of brick alone, at from 50 to 300.

If the material, in the most compressed edge, is not to be subjected to more than $\frac{1}{10}$th of its breaking weight, then brick should not be used of the proportions above.

23. If an iron band is placed around that part of the extrados, where the joints would otherwise open, it may entirely or partially prevent this opening. The arch then ceases to be strictly a voussoir arch, so that this device will not be further noticed.

24. We have been careful to expose this law of the decomposition of the resultant on a cross section in detail, since so many writers have fallen into error on this point: some applying the principles affecting a solid beam to the open joint, and thus discovering a veritable case of rotation when the line of pressures passes outside the "middle third" of the arch ring, due to supposed tensile forces; and others again rejecting the law of the trapezoid entirely, and falling back upon the theory of incompressible voussoirs.

It is needless to give all the various theories, even in outline, that have been proposed by so many able writers. Suffice it to say, that it is probably agreed, that the true solution of the arch is intimately connected with the law of its compressibility; or more plainly, the law of its deformation due to its elasticity.

The aim is, therefore, whilst not attempting a thorough solution, to endeavor to present clearly some important points bearing on this subject.

25. It is assumed as proved, that if, *in any completed arch, no joints open, that the actual line of pressures keeps within the middle third of the arch ring.* In fact, it is well to design the arch ring so as to satisfy this condition.

It does not follow, by any means, that if this actual line is found outside the middle third, that the arch will fail, as Rankine's eq. asserts. We have just seen that both strength and stability may be satisfied when this line approaches the edges at certain joints quite closely, so that other limits might, in many cases, with safety be instituted.

But it seems advisable, entirely from a practical point of view, not to have any open joints; it gives an appearance of insecurity and besides may leave too small a margin for shocks.

This principle does not apply to culverts, buried out of sight and never sustaining any shocks or much variation of pressure; hence it is extravagance to give them the same depth of keystone as a stone bridge. It is for the same reason proper to increase the depth of arch stones for a railroad arch bridge over the sizes usually employed in road bridges, as the live loads are heavier and move faster. For tunnel arches the judgment of the engineer must be largely exercised in allowing for the different thrusts of the various materials found in tunneling; and here it is better to be on the safe side and use a deep arch ring to allow for variations in thrust, especially if the soil is treacherous.

26. Let us recur again to the gothic arch, Fig. 1; which, as remarked, spread outwards about the haunches when set up on the solid piers. If with the hands the tops of the piers are moved inwards until the span is just 14 inches, the joints are all closed as stated in Part I, p. 50. The line of pressures is, therefore, confined to the middle third. If the top of the pier is pushed still farther in, the crown joint opens at the top, leaving at the bottom edge; and this is so, whether the other joints that may open are wedged up just to close or not.

Now there are some intermediate positions for the top of the pier when the horizontal thrust at the crown acts at different points along the joint: for as the top of pier is moved gradually inwards from its limiting position the thrust at the crown must travel *gradually*, down the joint, until it reaches the lower edge, when, of course, the lower edge alone bears.

It cannot jump at once from its highest to its lowest position.

This granted, a very important proposition is established: *that by cutting or fitting the arch stones in a certain manner that the line of pressures at the crown may be made to pass through any desired point of the crown joint.* Thus, in this case, with a span less than 14 inches, the thrust at the crown may be confined to any point of the crown joint between the top of the middle third limit and the bottom of the joint; and the voussoirs may be so cut that no other joint opens, even if the crown joint opens.

The same remarks apply to any other joint. A line of pressures may thus be made to pass at will through any point at the crown joint, and through some point below the previous one, generally on any other joint. Thus the arch may be pivoted at the crown and at the haunches, as see exps. 3 and 10 of Part I; or the line of pressures may be compelled to take the same position by properly chipping away part of certain joints; so that the true line of pressures is, after all, dependent on the mason. If he so cuts and lays the stones that on completion the bridge shows no open joints, as is the rule, then the line of pressure is somewhere within the inner third.

27. Next, suppose the piers removed and that the arch stands upon a firm support, the joints being closed as stated: where is the actual line of pressures? The line as first drawn corresponds to the principle of least resistance; but this cannot be the true one, *since, for no other reason, the consequent rotation about the upper edge of the crown joint and the lower edge of joints* 3 *or* 2, *would necessitate a rotation about the extradosal edge of joint* 4. But this last cannot occur, unless the line of pressures passes nearly through *a*, the outer edge of joint 4; involving a new curve of pressures, as shown by the dotted lines, *cra*, similar to the *actual* one drawn in Fig. 11 of Part I, referring to the first experiment, in which the joints opened as just described. In fact when the crown is lowered, the haunches must spread, and consequently the springing joint be most compressed on the extrados side. Now, in practice, this spreading always occurs at the haunches, so that the line of pressures there is below the centre line, whilst at the top and springing it is above it or outside of it.

If no joints open, as was the case, the line must keep within the inner third besides. Now, if the law of this deformation of the arch was known, the curve could be located; as it is, we can only approxmate to its true position, by noticing the manner in which arches settle, or fail as just shown. Now, it was proved by numerous experiments in Part I, *that when an arch failed by rotation, in every case the line of pressures corresponded to both the maximum and the minimum of the thrust.*

This is illustrated by figures 11, 12, 13, 23, and 24, of Part I, which figures refer to a few of the experiments made on the wooden arches. Now, it will probably be admitted that, with voussoirs that fit perfectly before decentering, and with incompressible abutments, that the deformation of the arch due to its compressibility, is in the same direction as that when the arch is at the limit of stability from a similar kind of loading; *i. e.*, the curve of pressures crosses the centre line the same number of times, und near the same places. Then it seems highly probable, under the conditions assumed, that *the actual line of pressures*, in such an arch, *is confined within such limiting curves, approximately equidistant from the center line of the arch ring, that only one curve of pressures can be drawn therein, corresponding, therefore, to the maximum and minimum of the thrust in the limits taken.*

Assuming this to be true, let us critisise the constructions given for the segmental bridge in Part I. Thus, in Figs. 9 and 10, the line of pressures is nearer the centre line than drawn. The curve for the limit of stability is represented in Fig. 13, when the weight is at the crown; and by Fig. 24, for a single weight on one haunch.

Now, according to the principles exposed above, a line of pressures for the bridge, Fig. 21, loaded eccentrically, should be drawn through the lower middle third limit, at joint 6, on the loaded side, the upper limit, at joint 6, on the unloaded side, and a point at the crown jotnt slightly lower than before. On a large scale drawing (3 feet to the inch), assuming the thrust at the crown joint to act 1.1 ft above the intrados, we find that the line of pressures passes above the middle third limits at joint 2, under the load 0.2 ft.—its max. departure—just touching the lower limit at joints 1 and 2, on the unloaded side. If preferred, the direction and amount of the thrust at the crown to pass through the given points on joints, 6, may be found by successive trials in place of computing them. Thus, assume the direction of the thrust: then find its amount to pass through the point on one joint 6, and use this amount in finding the center of pressure on the other springing joint, which should coincide with the point taken on that joint, otherwise, try again. Three trials only sufficed in this case.

From the above, we see that the depth of arch ring should be increased 0.2, in order that a line of pressure, giving the max. and min. thrust in the limits, may everywhere keep within the middle third. The recommendation, though, is repeated to increase the depth of the arch ring 0.5 foot, to allow for the influences mentioned in the next article.

If the object, therefore, is simply to investigate the stability of a proposed structure (which is indeed the real object of our investigation), it is evident that, *if any line of pressures can be inscribed within limits, so that no crushing occurs, that the arch is stable against rotation for statical loads.* For with less horizontal thrusts than that taken, the curve departs more and more from that which corresponds to the ultimate maximum and minimum, without which the arch cannot fail by rotation. As explained above, it is well to confine this line of pressures to the *middle third* of the arch ring, so that no joints open. If the line drawn corresponds to the minimum, but not to the maximum of the thrust within the limits taken, it is not the actual line of pressures (if the abutments are fit in, etc.), since this is probably contained within still narrower limits. In fact there will be an excess of stability in this case. In arts. 55, *et seq.*, the characteristics of the maximum and minimum thrusts will both be investigated in the most general manner.

Remark.—It should be observed that *if*, in a voussoir arch, there are no mortar joints, and the stonef are cut so perfectly that the compression is the same next the joints, as in the body of the stones, then *when* the pressure line keeps within the inner third, the conditions are exactly similar to the case of the solid or rigid arch "fixed at the ends." For the graphical treatment of this case, the reader is referred to Professor Greene's articles on this subject in *Engineering News* for 1877, in connection with Bell's article in VAN NOSTRANDS' MAGAZINE, Vol. 8; also to Professor Eddy's "New Constructions in Graphical Statics." The analytical treatment is given in full in Du Bois' "Graphical Statics." On testing the curve of pressures corresponding to the max. and min. of the thrust, if the segmental bridge with the eccentric load examined in Part I, by the three conditions, $\Sigma M=o$, $\Sigma My=o$, and $\Sigma Mx=o$, the center line of arch ring having been divided into 16 equal parts, it is found that the curve of pressures should be raised slightly, to satisfy these conditions, which are simply, that

the tangents at the springing are fixed, the span is invariable, and the vertical displacement of one springing above the other, equals zero. It seems, however, useless to enter into this refinement for actual bridges with mortar joints, rough beds, etc.; therefore, it is not mentioned further.

28. The abutments or piers have previously been considered as unyielding. If their tops lean outward, from a yielding of the foundation on the outer side where the greatest pressure is ordinarily thrown, or if the span lengthens from the compression caused, we see from the remarks on Fig. 1, that the actual line of pressures corresponds to a less horizontal thrust, so that it approaches the intrados at the springing, and, of course, retreats farther within the abutment at its base. If the abutment continues to yield, it approaches still more the contour curves at the crown and haunches, so that a sufficient yielding may cause the line of pressures to approach very nearly the very edges at those joints.

If the abutment is narrow, and especially if each course is built of a single block, like the voussoirs, for the thickness taken, it becomes really a continuation of the arch, so that all the principles of art. 27 apply; the base of the abutment becoming the actual springing joint.

Now, in practice, the abutments do yield somewhat; so that in most arches the actual line of pressures corresponds to a less thrust than as suggested in the previous article. In view of the fact that the arch-stones may not fit perfectly (for openings of the joints do occur sometimes, slate rock being driven in them), in addition to the foregoing, it seems impossible to say exactly where the true line of pressures is to be found. In most bridges the joints do not open, so that in them, it is confined to the middle third.

To be on the side of safety in designing piers or abutments, the maximum thrust in the limits may be used. If, however, the top of the pier leans, the thrust forcing it over becomes less, whilst the opposing thrust of the arch on the other side increases. When there are a series of arches resting on piers, the resultant on them due to dead load is vertical if the arches are all alike, or have the same horizontal thrust. In all cases, the pier should be investigated when one span is loaded and the adjacent one unloaded, exactly as shown in Part I., Fig. 22.

When the piers are very high, a series of arches are often placed below the first, which must, therefore, supply sufficient horizontal thrust to keep the line of pressures within the middle third of the horizontal joints of the piers from the lower arches to the base. The part of the piers between the series of arches are to be examined as before. Some magnificent structures have thus been built with tiers of arches. Instance of modern construction, the Morlaix Viaduct; also, the bridge over the Seine at the Pont du Jour, Paris. Often the piers of the upper series rest upon the arches beneath, which are generally of greater span than the upper series.

29. In addition to the influences exerted on the line of pressures by the cutting of the stones, a similar one producing similar effects is *Temperature.* Stoney says, "With increased temperature the crown rises and joints in the parapets open over the crown, while others over the springing close up. The reverse takes place in cold weather; the crown descends, joints over the springing open and those over the crown close. When stone or iron arches are of large span, these movements, from changes of temperature, will generally dislocate to a certain degree the flagging and pavement of the roadway above. This is very conspicuous in Southwark Bridge."

These combined influences, like the shocks due to rolling loads, can only be guarded against by empirically increasing the depth of the arch ring over that due to the statical loads, as recommended in art. 27.

Practical Conclusions.—In view of the combined influences of misfits, shocks, temperature, and yielding of abutments the true curve of pressures may be found in wider limits than those in which it has the characteristics of the max. and min. of the thrust in the arch ring.

If we design an arch ring then, in which a line of pressures corresponding to the max. and min. of the thrust can *just* be drawn in the middle third, some joints may still open from some of the influences mentioned above; therefore it is recommended to use somewhat narrower

limits than the middle third. Or better, require that a line of pressures may always be inscribed in the inner third, and increase the depth of arch ring empirically to allow for the other influences; since they are different for each bridge; dependent as they are upon the thrust, height of pier or abutment, kind of foundation, accuracy with which the stones are cut and other considerations detailed above.

INFLUENCE OF THE SPANDRELS.

30. The part played by the spandrels has not previously been enquired into. When the centers of an arch are struck, it is found, particularly for full center arches, that the crown descends and the haunches spread outwards. This spreading is *resisted* by the spandrels, which if solidly built, exert horizontal forces sufficient to prevent much lateral motion of the arch and thus help to keep the joints closed. If there is no tendency to spread in the arch, the spandrel exerts no horizontal resistance; it being simply a wall resting on a rigid arch ring, which thus cannot differ in its action from any other fixed foundation of the same shape. As generally built, with vertical joints next the arch, and even with inclined joints there, in most cases, there is no tendency to *slide*, which alone can cause an *active* horizontal thrust.

One author, M. Y. Von Villarceau, assumes that the spandrels exert an *active* horizontal thrust, proportioned, like liquids, to the depth below the surface! He thus obtains, after hundreds of pages of intricate calculations, "*the hydrostatic arch*"—one of the worst forms of arch a constructor could well choose. What a regret that such industry could not have been devoted to a more practical end.

Rankine's theory will be noted further on.

If in any of the experiments given in Part I, especially with the gothic arch where the spreading was very appreciable, a sufficient horizontal thrust was exerted by the hands or spandrel walls against the extrados, the arches, even under much greater loads would have been stable. Similarly in a stone bridge.

If the spandrel thrust, at different points was given, the investigation of the stability of the arch ring would be as simple as that for a culvert or tunnel arch given further on.

But it seems impossible to estimate it.

If each spandrel course was composed of one stone, and the yielding of these stones noted, then by the law, "*ut tensio sic vis*," we could deduce the horizontal resistance offered by each course. But if each course was made up of several stones, the forces corresponding to the compressions of the mortar joints would be too uncertain to rely upon, and thus the horizontal resistances could not be computed.

31. As, however, arches, such as the semi-circular, semi-elliptical, etc., that require a very effective spandrel thrust to maintain stability, are often built, it is necessary that the engineer comprehends all the reactions experienced in such arches, although he may not be able to precisely estimate them in tons, etc.

To this end, let us consider the semi-circular arch, Fig. 5, of 100 feet span, and 3 feet depth of keystone; the solid spandrel extending to 3 feet above the top of the keystone.

Divide up the spandrel by vertical lines, 5′ apart for 40′ from the crown, than 2′ apart for the next, 10′ and 1′ apart for the remaining 3 feet. The joints 1, 2, 3, ... are then drawn as in the figure.

Our object is first to ascertain the weight from the crown resting on any joint, and the position of the centre of gravity of this weight. The line of pressures, disregarding the spandrel thrust is then drawn as explained for Fig. 1.

In the following table the first column indicates the joint. In the next four columns, the upper numbers, opposite any joint number, refer to the trapezoidal figures; the lower numbers to the voussoirs on which the trapezoids rest:

(*See Table on following page.*)

We assume that the surface s of a *trapezoid* is equal to its horizontal width, w × mean height v; the latter being measured approximately from the top of the spandrel to the extrados along the medial vertical line.

For the *voussoirs*, surface $= s = w \times v =$ length measured along center line × depth (3′ in this case), see Part I., p. 78. Column c gives the horizontal distances from the crown to the medial vertical of the trapezoid (assumed to pass through its center of gravity), and to the

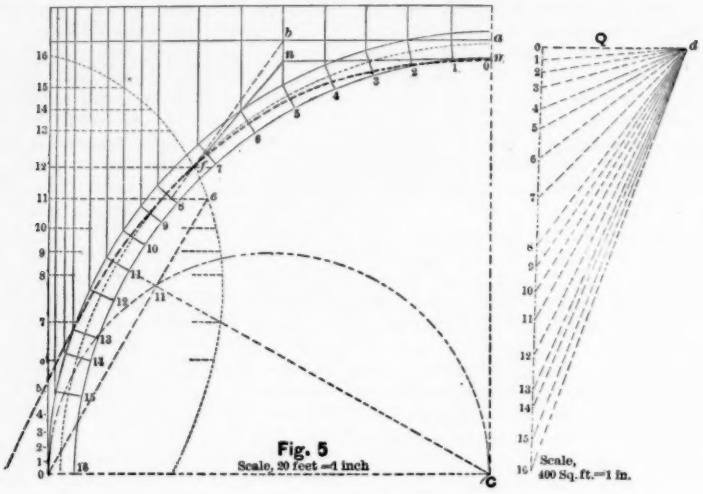


Fig. 5

Joint.	w	v	s	c	m	S	M	C
	5.	3.1	15.5	2.5	39.			
1	4.9	3.	14.7	2.5	37.	30.2	76	2.5
	5.	3.5	17.5	7.5	131.			
2	4.9	3.	14.7	7.4	109.	62.4	316	5.1
	5.	4.5	22.5	12.5	281.			
3	5.	3.	15.	12.3	184.	99.9	781	7.8
	5.	6.	30.	17.5	525.			
4	5.2	3.	15.6	17.1	267.	145.5	1573	10.8
	5.	8.	40.	22.5	900.			
5	5.3	3.	15.9	22.	350.	201.4	2823	14.
	5.	10.7	53.5	27.5	1471.			
6	5.75	3.	17.2	26.8	461.	272.1	4755	17.4
	5.	14.2	71.	32.5	2307.			
7	6.2	3.	18.6	31.7	589.	361.7	7651	21.1
	5.	18.6	93.	37.5	3488.			
8	6.87	3.	20.6	36.6	754.	475.3	11893	25.
	2.	22.3	44.6	41.	1829.			
9	3.1	3.	9.3	39.9	371.	529.2	14093	26.6
	2.	25.	50.	43.	2150.			
10	3.3	3.	9.9	41.9	415.	589.1	16658	28.3
	2.	27.9	55.8	45.	2511.			
11	3.7	3.	11.1	43.8	486.	656.	19655	30.
	2.	31.4	62.8	47.	2952.			
12	4.3	3.	12.9	45.8	591.	731.7	23198	31.7
	2.	35.9	71.8	49.	3518.			
13	5.13	3.	15.4	47.7	735.	818.9	27451	33.5
	1.	39.9	39.9	50.5	2015.			
14	3.23	3.	9.7	49.1	476.	868.5	29942	34.5
	1.	43.5	43.5	51.5	2240.			
15	4.2	3.	12.6	50.2	632.	924.6	32814	35.5
	1.	49.	49.	52.5	2572.			
16	10.	3.	30.	51.3	1539.	1003.6	36925	36.8
			1003.6		36925			

center of gravity of the voussoir respectively. The product, s. $c=m=$moment of s about the crown. Column s now gives the area of the trapezoids and voussoirs from the crown to any particular joint, and is formed by the successive

addition of the numbers in column *s*. Similarly M is formed from *m*. The quotient $\frac{M}{S}=C=$horizontal distance from the crown to the center of gravity of the surface S corresponding to the same joint. Thus, for joint 8, the sum of the moments of each trapezoid and voussoir from the crown to joint 8=M=11893; the sum of their area=S=475.3; hence by mechanics $C=\frac{M}{S}=25$. If we take a slice of the arch of the width unity, then S will represent the corresponding volumes,and is proportional to their weights.

The whole surface of the arch and spandrel = rectangle − quadrant $=56\times53-\pi\frac{25^2}{4}=1004.5$, differing 0.9 square foot from the value found approximately in the table.

32. Let us now pass a curve of pressures through a point *a*, 1′ below the extrados at the crown, and a point 1′ from the intrados along joint 8.

To do this we lay off on the horizontal through the upper point, $\overline{ab}=C=25'$ to the left of the crown. From the point *b* so determined, draw a straight line to the lower point at joint 8. This line gives the resultant on joint 8 in position and direction. Next, laying off to scale in the force diagram to the right, the surfaces S in order, and drawing through 8 a line ∥ the direction of the resultant just found, it will cut off on a horizontal through *o* the value $\overline{od}=Q$ of the horizontal thrust at the crown.

Drawing lines through 1, 2, . . . (force diagram) to *d* so found, we have the directions and magnitudes of the resultant on joints 1, 2, . . . represented by their lengths. To find the center of pressure on any joint as *e. g.*, 5, we lay off C=14 on the horizontal $\overline{ab}$. From the point thus found draw a line ∥ $\overline{ds}$ of force diagram; where it intersects joint 5 is the center of pressure on that joint: for the thrust Q at *a*, combined with the weight resting on joint 5, acting at its center of gravity must give the resultant on joint 5 in position, magnitude and direction. Similarly for other joints.

The broken line traced through these centers of pressure—not drawn in the figure to avoid confusion—is the "*line of pressures.*" In this case it everywhere keeps within the middle third of the arch ring, except at joints 15 and 16. At joint 15 it passes in the arch ring 0.6 feet from the extrados; at joint 16, 1.9 feet outside of the extrados. [If there was no spandrel, the true line of pressures must lie in the arch ring, if possible, and satisfy the conditions of art. 27.]

33. At joint 7, the center of pressure is at the middle of the joint; the line of pressures then goes below the center line of the arch ring, approaches nearest the intrados at joint 8; at joint 13 it again crosses the center line, and keeps above it to the abutment. Joints 8, 9 and 10 are the most compressed on the intrados side. Joint 8 is often called the joint of rupture.

34. Now if this were the true curve of pressures, the effect of the compression, not being uniform on the joints, would be to lower the crown and spread outwards the haunches (see art. 28). So that if the spandrels were solidly built up to joint 7, they would offer horizontal resistances, in addition to their vertically acting weights to partially prevent the deformation of the arch ring.

Let us suppose, for an instant, that the spandrels are absolutely incompressible; the arch, then, cannot change shape where the tendency to spread occurs; therefore, the compression must be uniform on such joints; *whence the actual line of pressures must pass through the centers of those joints.*

This compression along the arch ring shortens it slightly, but we shall neglect this shortening. Our untrue hypothesis conducts us to the following construction, similar to the one given in Rankine's Civil Engineering, article 138: lay off, on the extreme left vertical, the loads from *o* (Fig. 6) upwards. Now, *assume* that the curve drawn tangent to the resultants on the joints coincides with the "line of pressures" (which hypothesis is sensibly incorrect as we approach the springing). Draw $\overline{oe}$ parallel to the center line at joint 11—the construction shown effects this easily, by drawing $\overline{o,11}$ through the inter-section of $\overline{c,11}$ with the semi-circle. Next draw a horizontal line through 11 on the scale of loads, to the intersection *e* with

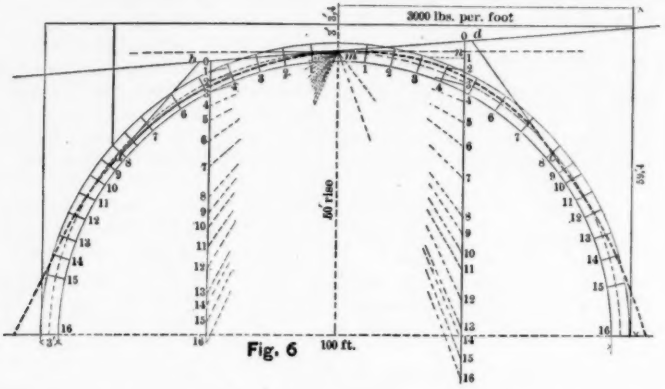


Fig. 6

the line just drawn; then $\overline{oe}$ represents the magnitude and direction of the resultant at joint 11, whose two components $\overline{o,11}$ and $\overline{e,11}$ are respectively, the load from the crown to joint 11, and the *total horizontal thrust exerted below* joint 11.

On repeating this construction for each joint, we find the horizontal thrust exerted below each joint; the horizontal thrust, then exerted upon a single voussoir, as that between joints 11 and 12, by the spandrels, is thus the difference between the line e, $\overline{11}$ and the horizontal f, $\overline{12}$. At joint 8, the horizontal thrust obtains its maximum; and above this point, in this case, the spandrel would have to exert tensile forces to cause the center line to become the true line of pressures; but as it cannot do this, the horizontal thrust from joint 8 to the crown is constant.

35. If an accurate construction should be desired, to ascertain the horizontal force supplied by the spandrel at the extrados of each voussoir, in order that the center line may be the curve of pressures, from the joint where spreading first occurs to the abutment, we may proceed as follows: assume the thrust on that joint to pass through its center; then having assumed the position at the crown of the horizontal thrust, we find, as in art. 32, the magnitude and direction of the resultant on the joint considered. Combine this resultant with the weight of next voussoir and load, acting through their common center of gravity, and the resultant so found with such a horizontal force, acting through the middle, approximately of the extrados of the voussoir, as to cause the final resultant to pass through the center of the next joint. This construction may be continued to the abutment.

If a force polygon is drawn to one side, the amounts of the horizontal thrusts supplied to each voussoir by the spandrel becomes evident.

36. Continuing the construction of art. 35, we draw a tangent to the center line at joint 8, to intersection n with the vertical through the center of gravity of the load from the crown to joint 8, and from this point draw $\overline{mn}$ horizontal, to intersection m with the crown joint. From m the center of pressure on the crown joint, the curve of pressures to joint 8 is drawn as before explained in art. 32. It is shown by the dotted line through m which point is 0.2 feet below the crown. The curve is continued below joint 8, supposing *no* spandrel thrust, and cuts the springing joint 4.5 feet to the left of the extrados.

On the supposition of incompressible spandrels however, the true curve is that drawn through m to joint 8; then it follows the center line. Rankine indeed, asserts that a linear arch parallel to the intrados and drawn within the middle third is the true curve, whence n and m may be slightly raisedor lowered. (See Rankine's Civ. Eng., art. 285.)

This cannot be if the arch is to perfectly preserve its figure (art. 34). Again, for compressible spandrels, the line of pressures about joint 8 *must* lie below the center line, *never* above it (art. 28).

37. But is this the true curve? Decidedly not. If there were no spandrels, and the abutments yielded sufficiently, the curve would be somewhat as drawn in art. 32, tending to flatten the arch from the crown to joint 7; then to render it more convex; and below this

point causing the greatest compression to occur at the extrados.

Now, *theoretically*, with spandrels built in the usual manner, when the centers are struck, the tendency of the arch to spread at the haunches *causes* the compression of the spandrels, which thus, in partially resisting this spreading, put forth horizontal forces. The true line of pressures then between joints 7 and 13, about, must pass *below* the center line; below joint 13 approximately, it keeps to the left of the center line. The latter follows from the fact that any spreading at the haunches is accompanied with a diminished compression of the intrados at joint 16, (see art. 27).

Again, the spandrel thrust must be greatest where the spreading is greatest; whence from joint 8 the spandrel thrust necessarily diminishes down to the springing or near it, where it ceases. No spandrel thrust is experienced above joint 8 about, if the spandrel moves as one mass.

In practice, the spandrel is not solidly built above joint 8, except on the faces of the arch, so that on that account, the thrust above joint 8 will be small if any.

38. *Practically*, the first solution above conducts to this absurdity; the arch should be unstable because m lies below the arch ring, but the depth of keystone was obtained from a comparison of examples in actual practice, so that no engineer would believe that a full center arch of 100 feet span and 3 feet depth of keystone should be unstable. Thus, from Rankine's formula, founded entirely on practice, the depth of keystone should be, for a single arch $\sqrt{.12\times r}=2.45$ feet, or for an arch of a series $\sqrt{.17\times r}=2.9$ feet; r being the radius at the crown, 50 feet in this case.

Again, *in practice*, the crown falls on decentering; hence it seems probable that the true line of pressures there is *above* the center line, not below it.

39. *Now if the joints keep closed, the actual line of pressures probably passes above the center line at the crown; between the center line and the lower middle third limit at joint* 8 *about; from this point it again approaches the center line, crossing it about joint* 13 *and passing near the outer middle third limit at the springing joint, keeping throughout within the middle third of the arch ring. The spandrel then, from the point of greatest spreading to, or near, the abutment, must exert the least horizontal resistances that will effect this object. Some idea of their magnitudes could be gained from the construction of art.* 35, *if we knew two points, say at the crown and joint* 8, *through which to pass the curve of pressures, assuming its position below joint* 8.

If this be true, then the thrusts exerted by the spandrels are evidently much less than as given by Rankine's construction, and the spandrel in a semi-circular arch does not sustain the whole of the horizontal thrust. Rankine's theory is the only one, except Y. Von Villerceaus, that has yet been proposed to evaluate the spandrels' influence.

40. If the top of the backing of an arch is sloped downwards from the arch, it may not be capable, near the top, of exerting much horizontal thrust.

If we *knew* the total spandrel thrust down to a certain horizontal joint, the weight of the spandrel above this joint multiplied by the coefficient of friction of stone on stone is the force that resists the sliding tendency; so that the height of spandrel, on this supposition, is easily computed.

As Rankine's construction gives an excess of spandrel thrust, it may be used to evaluate the least height of backing, both loose and solid, to be used.

41. Whatever doubt may exist as to the precise *measure* of the forces exerted by the spandrel, its important action in preventing deformation of the arch ring from a theoretical stand point is rendered plain by the above discussion; and the locus of the true curve of pressures is more precisely ascertained than hitherto; which was the object to be accomplished in the present instance. It is usual with constructors to strike the centers, after the keystone is driven in and the backing carried up such a distance above the "joint of rupture" (as joint 8 is often called) that the arch ring will be stable when the supports are removed. This would seem to be a matter to be determined from practice; though a curve of pressures can be used in approximately testing the stability of the unfinished arch.

It may be observed that, if the resultant at joint 8 maintains its position and

direction approximately, that the center of pressure at the crown for the completed arch will be lower than for the unfinished arch, *if* the vertical through *b* moves to the left as the arch is completed; otherwise the reverse happens.

42. *Width of Piers and Abutments.*—For full security, in these arches where the spandrels exert a marked influence, the horizontal thrust at the crown may be taken as acting at the lower middle third limit, whence the center of pressure at the base of an abutment is determined exactly as in art. 2, Fig. 1, (also, see art. 28). The right half of the arch being supposed removed and Q being applied at the crown to produce the same effect; its combination with the weight of the semi-arch and abutment must give the resultant acting on the base of the latter, irrespective of internal actions, such as the real distribution of the spandrel, thrust, &c. For the piers, suppose again, for safety, that Q acts at the lower middle third limit at the crown; then find the resultants acting at the level of the springing, due to the arches on both sides of a pier; on combining them with the weight of pier, acting at its center of gravity, the center of pressure at the base can be found. It is a good practical rule to limit this center of pressure in both abutments and piers to the middle third of the base (see art. 9); and to cause it to approach the center as the foundation becomes more insecure.

43. Let us next suppose the full center, Fig. 6, loaded with cars weighing 3000 pounds per lineal foot from the crown to the right abutment. If this load bears upon a width of six feet, it is equivalent to a layer of stone of same density as the bridge (150 lbs. to the cubic foot), 3.4 feet high, as represented in the figure.

The following table for the right half of the arch is made out exactly as explained in art. 31, only the voussoir numbers, for each joint, are placed above the corresponding spandrel numbers. Now let us pass a curve of pressures $\frac{1}{12}$th depth of arch ring below the center line

	s	*c*	*m*	S	M	C
	14.7		37			
1	32.5	2.5	81	47.2	118	2.5
	14.7		109			
2	34 5	7.5	259	96.4	486	5.
	15.		184			
3	39.5	12.5	494	150.9	1164	7.7
	15.6		267			
4	47.	17.5	822	213.5	2253	10.5
	15.9		350			
5	57.	22.5	1283	286.4	3886	13.6
	17.2		461			
6	70.5	27.5	1939	374.1	6286	16.8
	18.6		589			
7	88.	32.5	2860	480.7	9735	20.2
	20.6		754			
8	110.	37.5	4125	611.3	14614	23.9
	9.3		371			
9	51.4	41.	2107	672.	17092	25.4
	9.9		415			
10	56.8	43.	2442	738.7	19949	27.
	11.1		486			
11	62.6	45.	2817	812.4	23252	28.6
	12.9		591			
12	69.6	47.	3271	894.9	27114	30 3
	15.4		735			
13	78.6	49.	3851	988.9	31700	32.
	9.7		476			
14	43 3	50.5	2187	1041.9	34363	33.
	12.6		632			
15	46.9	51.5	2415	1101.4	37410	33.9
	30.		1539			
16	52.4	52.5	2751	1183.8	41700	35.2
	1183.8		41700			

at the crown joint and joints 8, through m, a and c. The thrust at the crown is generally inclined. Let us deduce some general formulæ to enable us to find it.

44. Call P_1 = weight from crown to joint 8 on left,
a_1 = its lever arm about a,
P_2 = weight from crown to joint 8 on right,
a_2 = its lever arm about c,
Q = horizontal component of thrust at m,
b_1 = lever arm about a
b_2 = " " " c
P = vertical component of thrust at m, considered positive when it acts downwards as regards pressure from the right half of the arch upon the left.

g_1 and g_2 are the horizontals from a and c respectively to the vertical through the crown.

Now suppose the right half of the arch removed and its effect replaced by P and Q acting at m; we have taking moments about a.

$$a_1P_1+g_1P=b_1Q. \quad . \quad . \quad . \quad (5)$$

Next, conceive the left half removed, &c., and take moments about c.

$$a_2P_2-g_2P=b_2Q. \quad . \quad . \quad . \quad (6)$$

Eliminating Q, we have

$$P=\frac{a_2b_1P_2-a_1b_2P_1}{g_1b_2+b_1g_2} \quad . \quad . \quad . \quad . \quad (7)$$

From (5)

$$Q=\frac{a_1P_1+g_1P}{b_1} \quad . \quad . \quad . \quad . \quad (8)$$

See more general formulæ in Part I, art. 12, and in art. 63 following.

45. From the tables of arts. 31 and 43, we have $P_1=475.3$, $P_2=611.3$; and their centers of gravity are distant from the crown, respectively, 25. and 23.9 feet.

From the drawing, we have thus, $a_1=13.7$, $a_2=14.8$, $b_1=b_2=17.5$, $g_1=g_2=38.7$, whence,

$$P=\frac{-a_1P_1+a_2P_2}{2g_1}$$

$$=\frac{-13.7\times475.3=14.8\times611.3}{2\times38.7}=32.8$$

$$Q=\frac{a_1P_1+g_1P}{b_1}$$

$$=\frac{13.7\times475.3+38.7\times32.8}{17.5}=444.6$$

From m lay off to the right, horizontally, $Q=444.6=\overline{mn}$; then vertically upwards, $P=32.8=\overline{no}$: $\overline{om}$ represents the resultant at the crown joint. Now, lay off the force lines $o, \ldots .$ 16 from colums S in the tables; so that $\overline{m1}$, $\overline{m2}, \ldots$ now represent the directions and magnitudes of the resultants on joints 1, 2, ... right and left of the crown. Their positions are found as follows: draw a horizontal through m, and lay off on it, the numbers in column C; the first table referring to the left half of the arch, the last table to the right half.

From the points so found, draw vertical lines to intersection with $\overline{mo}$, produced if necessary; which thus give the points where the inclined thrust at m is to be combined with the weight from the crown to any joint, to find the resultant on that joint; whose intersection with it is thus the center of pressure for that joint.

Thus, P, acts 25′ to left of m: lay off 25′ on the horizontal through m, then drop a vertical to intersection b with mo; then draw $\overline{ba} \parallel m8$ of force line for left of arch, to find a the center of pressure for joint 8. Similarly d and dc are found for joint 8 on the right. These should be the first constructions made to test the values of P and L found, which correspond to the line of pressures passing through a, m and c.

The line of pressures thus drawn passes below the middle third of the arch ring, on the unloaded side, the following amounts in feet: at joints 2, 3, 4, 5 and 6, .3, .4, .3, .2 and .1 respectively; it then crosses the arch ring, passes above the middle third about joint 12, and cuts the springing joint 4.5 feet outside of the arch ring.

On the loaded side it passes above the middle third 0.1 at joints 4 and 5; then across the center line and is just tangent to the lower middle third limit at joint 10, below which it again crosses the arch ring and passes into the abutment, cutting joint 16 about 3 feet outside of the arch ring.

46. On the unloaded side this curve below joint 8 follows very closely the curve drawn in fig. 6; so that if horizontal forces, as large as the ones supposed supplied by the spandrels by Rankine's construction are applied here, the line of pressures will coincide nearly with the center line. On the loaded side the line of pressures touching the lower

middle third limits at joints 9 and 10 would be forced too far in by the supposed horizontal thrusts. As mentioned in art. 39, this spandrel thrust is probably much less than given by the above construction; thus if c is at the center about, and a at the lower limit, the thrust at the crown being raised slightly above the center, the spandrel thrusts, necessary to keep this line of pressures within the middle third say, are nearer the true ones than as given by the previous construction. It seems, at present, impossible to locate exactly the true curve of pressures when spandrel thrusts are exerted.

47. If, however, the abutments are large enough and unyielding, and the spandrels sufficiently strong to resist the thrust, the arch *cannot fail* by the sinking of the crown, which is the usual method of failure of full center arches. Thus Gauthey says (see extract in Haupt's Bridges, p. 126), "let *cvc'*, Fig. 7,

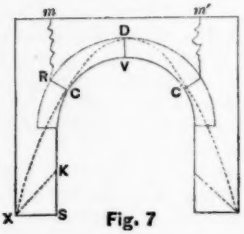


Fig. 7

be the intrados of any arch, whether semicircular, elliptical, gothic or composite. Let D be the crown of the extrados, or back of the arch, which is supposed to be filled up level with the haunches at m and m'. If a weight be placed upon the crown too great for it to bear, it yields, and the arch stones open beneath at the crown, while the extrados is found to open at some point on each side; either at the spring, if it be a flat arc of a circle, or about 30 degrees of a semicircle, or at various other points if it be composed of arcs of circles, tangent to each other, and of various rises, whether $\frac{1}{4}$ or $\frac{1}{3}$ or $\frac{1}{2}$ of the span; and the arch *only falls*" (italics our own) "by pushing aside the abutments at C and C', the opening at R extending itself up to the top at m and m'. It has, moreover, been observed that when the abutment gives way, it leaves a portion of itself standing, viz., XKS, the line XK being at an angle of 45° with the horizon, which only adheres by the strength of the mortar or cement made use of."

This last fact should be observed in designing pillars of any kind. The line of pressures for the kind of rotation just mentioned is shown by the dotted line.

If the arch ring has a very small depth some of the voussoirs may crush; or again, the arch may fail by rotation by the *crown rising* and the haunches falling in, as in the bridge over the Tâfe (see art. 51). Eccentric loads, as we saw in Part 1, cause the haunch under them to fall, and the opposite one to rise. In these cases the rising of the crown—we repeat, the only way in which a bridge can fail with solid, immovable abutments, and spandrels sufficient to resist the thrust against them—tends again to relieve the spandrels of a certain amount of strain, thus causing the curve of pressure to *rise* at the crown, with a less horizontal thrust, and thus partially counteract the tendency towards rotation about the intrados at the crown.

The depth of voussoir, in this case, can only be determined by an empirical rule founded on practice, as given below. A rolling load evidently causes different spandrel resistances from the dead load, though Rankine's construction does not make any difference for the unloaded side; another proof of the incompleteness of his theory.

48. Let us investigate the part *amc* of the bridge as though it was a *segmental bridge*, resting upon fixed abutments at a and c. It is only needful to examine it for an eccentric load (see Part I.) Pass a curve of pressures through the upper middle third limit at joint 8 on the left, the lower limit at joint 8 on the right, and 1.25 ft. above the intrados at the crown joint. We find on a drawing of 3 ft. to the inch, $g_1=39.25$, $g_2=38.48$, $a_1=14.25$, $a_2=14.58$, $b_1=17.16$ and $b_2=17.81$. As before, $P_1=475.3$, $P_2=611.3$: whence, by eq. 7,

$$P=23.8,\ Q=449.1.$$

The line of pressures drawn with these values keeps everywhere within the middle third, barely touching the lower limit at joint 2 on the left, and passing 0.16 ft. inside of upper limit at joint 3 on the right, and corresponding (art. 27)

nearly to the maximum and minimum of the thrust in the limits chosen.

The span of this arch is 75.45 feet, its rise being 17.2 feet, between $\frac{1}{4}$ and $\frac{1}{5}$ of the span. The deformation is small for this segmental bridge, so that the spandrel resistance may be neglected, or rather regarded as simply adding to the stability of the bridge, already safe, unless from the dynamical effects of moving loads. Concentrated loads on one side (as in art. 12, Part I.) should next be tried, in positions that cause the most hurtful effects. This influence, together with the influences of art. 28, may cause an increase of depth of arch ring of half a foot over the three feet.

49. Recurring to the semi-circular arch, Fig. 6, it is evident that for an unsymmetrical load, the spandrel on the opposite side to the load will exercise a thrust for a greater height above the springing than for a uniform load, as compare the line of pressures in the two cases.

It may be asked, why does not the part of the arch below the joint of rupture, *a*, Fig. 6, act as a part of the abutment simply; so that if the part *amc* satisfies the conditions of stability, when it is treated as a segmental bridge, why should not the whole bridge be stable?

Rankine's empirical formulae for the depth of keystone, $\sqrt{cr}$, r being the radius of the arch, and c a constant, seems to be founded on such a hypothesis; for by it, the depth of keystone is the same for spans of any length, provided the radius is the same. Thus, if $r=50$ feet, as in Fig. 6, this depth is the same for any span between 100 and 0 feet!

The above query may be answered thus: the actual line of pressures in an arch bridge like Fig. 6, is dependent upon the form of the arch below the points *a* and *c*, since the deformation of this part induces the spandrel thrusts (art. 37) which influences the position of the true line of pressures.

In fact, suppose that the line found in Fig. 6, on the supposition of no spandrel thrusts, to be the true one for a segmental bridge *amc* of span *ac:* by the reasoning of art. 46, the actual spandrel thrusts exerted below *c* would force this line out of the middle third, certainly, and most probably out of the arch ring, whereas, the spandrels are supposed to allow no joints to open at least. Trautwine's empirical formulæ for depth of key in feet, *d*, is,

$$d=\frac{\sqrt{r+\text{half }\overline{\text{span}}}}{4}+.2;$$

and is more agreeable to theory than Rankine's; although for railroad bridges it gives too small values; at least for bridges of 50′ to 75′ span, and rises $\frac{1}{3}$ to $\frac{1}{5}$ span, as for the two segmental bridges examined in art. 48, and in Part I., art. 12. It is evident again, from the reasoning of art. 28, that it would be advisable to increase the depth of arch ring, the smaller the ratio of width to height of abutments; and by the same rule, an arch in a series should have a greater depth of voussoirs, as recognized in Rankine's rule for that case.

50. Having resigned the above arch, that requires a spandrel thrust to keep it from falling, to the domain of empiricism, it may be asked, if by some device of construction, whether it may not be brought within the limits of a strict investigation? Plainly, if the depth of arch stones be increased towards the abutments, so that a line of pressures, with a constant horizontal thrust, can always be inscribed within the middle third, the arch will be stable; and the spreading will be so much diminished that the arch will require but little spandrel thrust to cause stability, so that its influence may be neglected in constructing the line of pressures.

This increase in the depth of arch stones is earnestly recommended; as well as the continuation of the arch ring into the abutment, when, as in segmental bridges, there is some danger of sliding at the springing.

If necessary, the increase in depth of the arch ring may be made up of several stones. They should, of course, break joint with stones above and below them, and be well bonded with the spandrels.

The last pier of the first fixed bridge across the Lim Fjord, an arm of the sea stretching right across the Danish continent from east to west, has, according to recent news, now been completed. The new bridge will connect Aalborg on the south side of the Fjord, with Norresundby on the north, and it is thought that it will be opened for traffic during the autumn.

ELECTRIC LIGHTING.

Compiled from various sources.

When Davy, in 1813, first produced the electric light between the charcoal points of two conducting wires, it was at once proposed to use it for all the various purposes of illumination, simply because in volume of light it excelled all other known sources of light. But it became soon evident that great obstacles presented themselves to the general application of this light. Davy had used for its production a gigantic galvanic battery of 2,000 elements; the lengths of charcoal were quickly consumed in the enormous heat of the arc of light, and had to be moved towards each other, if the light was not to be extinguished, in the same degree as they were consumed; and, finally, the light was concentrated upon a small space, the attempt of dividing it into several lights having also failed. A step in advance was made when, using stronger acids, new elements were constructed which gave more powerful currents; it was possible to produce with from forty to fifty of such elements a beautiful electric light; and the efforts were successful of maintaining, by means of the current itself and the clockwork suitably connected with it, the charcoal points at the distance required for the supply of a continuous light for hours without manual intervention. But notwithstanding the great improvement of the galvanic elements, the cost of the light produced by them was still too great, and the management of the batteries too troublesome, for its general practical application for the purposes of illumination to be thought of. Its use was confined to some scientific purposes, and the occasional production of great effects of light in theatres, &c.

Faraday, in 1831, found a new way of generating electrical currents, by moving magnetic poles towards closed wire coils, or *vice versa*, and since then opened out to physicists and engineers a new, wide field to produce by the suitable construction of magnetic machines powerful electrical currents, and with these the electric light without batteries, consequently in a purely mechanical way. Step by step progress was made in this direction, while on the other hand it was tried to improve the electric lamps, with a view of keeping the charcoal rods at the same distance during combustion. Already in 1850, we find both, the magnetic machine and the electric lamp, improved to such a degree as to provoke serious thought of applying the electric light to lighthouses, for illuminating large spaces of ground for war purposes, &c.; and yet it was not until the end of 1863 that the first electric light was shining in the lighthouse of Cape la Hêve, near Havre.

In the meantime, Dr. W. Siemens, of Berlin, already then well known for his eminent services rendered to telegraph engineering, had invented his induction cylinder, and had employed it both for the working of the telegraph and in the construction of a new magnetic machine, small but powerful; and Wilde (1866) undertook to make use of the Siemens machine in combination with a steam-engine of fifteen horse-power in such a manner as to be able to produce electrical currents and the electric light of a power until then never thought of. But Wilde's machine was very expensive both as regards construction and working, and found but little favor in consequence.*

In the same year, Dr. Siemens discovered the dynamo-electrical principle, or the manner of changing any description of mechanical work without steel magnets into electrical currents. It is well known that there are no different descriptions of forces in nature, but only various forms of one and the same fundamental force, the essence of which is movement, either the movement of larger bodies (mechanical work) or the movement of the smallest particles and the ethereal body of the universe (heat, light, electricity). For that reason it is possible to transform any of these forms of force—for instance, the muscular pow-

* Mr. Wilde has obtained on interim injunction, restraining Messrs. Wells, of Shoreditch, from using the light which they have adopted at their premises, on the ground that it involves an infringement of his patent.

er of living beings, steam-power, water-power—into heat or light or electricity. With this invention of Siemens, galvanic batteries were doomed, and a means offered to engineers of producing electrical currents of a power hitherto unknown, and on proportionately small machines.

Of the various magneto-electric machines that have been brought forward, that of M. Gramme is the most generally used. As soon as his machine became practicable, an English company purchased the English and American patents, the Société d' Encouragement awarded a gold medal to the inventor, and a large number of manufacturers ordered the apparatus. The exhibitor has been awarded prizes at the exhibitions of Lyons, Vienna, Moscow, Linz, and Philadelphia; and now about 500 machines of his construction, with magnets or electro-magnets, have been delivered, and the demand for them is still increasing. Electric lighting, which before M. Gramme's invention did not exist, speaking industrially, is at the present day within the domain of things practical. It is not within our scope to give a detailed description of his machine; it is sufficient for our purpose to state that it furnishes continuous electric currents, the direction and intensity of which may be changed. The electric current having been generated is conducted through an insulated wire to a perpendicular rod of retort carbon, the point of which is placed exactly above the point of a similar rod, the distance between the points being less than a quarter of an inch. This intervening space is the electric arc, and the current passing through that interval from the rod above to that below heats the points to a state of intense incandescence, producing the electric light. The current passing only in one direction—from the upper or positive pole to the lower or negative pole—consumes the positive at double the rate of the negative, and consequently the distance between the points has to be continually readjusted by clockwork and a magnet, constituting the regulator or electric lamp.

Davy made use of rods of wood carbon extinguished in water or mercury. They burned with great brilliancy and very regularly, but they were consumed very rapidly, so as to be useless for practical purposes. Now retort carbon, the deposit collected from the walls of gas retorts, is employed. It is much more dense than wood carbon, and resists for a long time the destructive action of the voltaic focus. Here, also, there is room for improvement, for retort carbon is not all that could be desired, but it gives satisfactory results in most of its applications.

The best known apparatus for regulating the consumption of the carbons are those of Archereau, Lacassagne and Thiers, Gaiffe, Foucault and Duboscq, Hefner-Alteneck, and Serrin. The first two are no longer in use.

By the side of the electric lamp with regulating apparatus for the carbon rods as they become consumed, Jablochkoff's candle has also become the material for electric lighting. M. Jablochkoff's light completely suppresses the regulator. His invention was presented to the Academy of Sciences in October, 1876. The carbons, instead of being opposed, are placed side by side, and are separated by an insulating fusible substance. When the current begins to pass, the voltaic arc plays between the ends of the carbons. The layer of insulating matter melts, volatilises, and the double rod of carbon slowly consumes, exactly as the wax of a candle progressively exposes its wick. M. Jablochkoff now burns in his candles, as they are called, powdered asbestos. It seems as if the interesting labors of M. Jablochkoff will have practical result, and that they will increase the domain of electric lighting, for his lamps are now largely used. In Paris, besides numerous larger electric lamps, at the present time, there are burning many Jablochkoff candles, of which we may mention eight on the Place de l' Opera, twenty-four in the Avenue de l' Opera, eight on the Place du Theatre Francais, six at the Palais Bourbon along the front facing the Place de la Concorde, seventy in the Grands Magasins du Louvre, eight in the shops of the Belle Jardiniere, sixty in the Concert de l' Orangerie des Tuileries, thirty-two in the interior of the Hippodrome.

Returning to the Gramme machine, it appears that the first light machine constructed by M. Gramme fed a regulator of 900 Carcel burners; its total weight

was over 2,000 lbs. This machine served for a long time for the experiments on the clock-tower of the Houses of Parliament at Westminster, The fault found with this machine was that it became heated, aad gave sparks between the metallic brushes and the bundle of conductors on which the current was collected. This, however, has not given rise to any serious inconvenience during five years. M. Gramme's next machine was less powerful, of a power of only 500 burners, and consequently of smaller dimensions. When a current is sent into two regulators, each will give 150 Carcel burners. This apparatus has been introduced on board the *Suffren* and the *Richelieu*, of the French navy; on the *Livadia* and the *Peter the Great*, of the Russian Navy; it is employed by several Governments for service in fortified places. This machine is described as excellent, but its luminous intensity is slightly feeble when the atmosphere is foggy; its price, however, is somewhat high. The inventor has improved upon this machine, and constructed one which, when coupled in tension, gives a luminous intensity of 800 Carcel burners at 700 revolutions per minute, and, if coupled in quantity, 2,000 Carcel burners, with 1,350 revolutions per minute. It has been adopted by the French Ministry of War, by the Austrian navy and artillery, by the Norwegian, Turkish, and other Governments. By further simplification, M. Gramme has been able to introduce a machine most suited for industrial purposes, large workshops, and large covered spaces.

As has already been remarked, the electric light may be advantageously employed in a large variety of works, for it admits of obtaining a great quantity of light at a smali expense. By its means, the loading and unloading of cargoes, the mounting of machinery, carpentry, weaving, dyeing, and similar trades may be carried on by night just as well as in broad daylight. It is necessary, however, to employ two machines, in order that the light of the one should counteract the shades thrown by the other. It has been found by experience that the naked light may be employed, the workmen themselves having asked for the removal of the opal globes which it was thought at first necessary to usa. The electric light preserving the tints of colors, this property has been utilized with success by several dyers for standardizing their colors by night.

The electric light is most effective for high rooms; when ceilings are of a less height than twelve feet its introduction becomes more difficult. As a rule, there may be conveniently lighted with a single apparatus about 5,000 square feet of fitters' shops, lathe-shops, tool-shops, and modeling-rooms; half that space in spinning-mills, weaving establishments, and printing-rooms; and about 20,000 square feet of yard, court-yard, dock-yard, quay, and open air works.

In a country like the British Isles, where the safety of the mercantile marine and the navy depends so very much upon the amount of security with which ships may enter ports, and the care bestowed upon keeping up an effective system of lighthouses to warn the navigator against approaching dangerous coasts, the electric light would be sure to prove a welcome auxiliary in effecting those objects; and so in reality it has, being now employed at many of the stations. It renders visible at night, at distances varying from 2,000 to 6,000 yards, objects such as buoys, ships, coasts, etc. The electric light was first applied to lighthouses in 1863. In that year trial was made with an Alliance machine at the lighthouse of La Hève, near Havre, the results being so satisfactory that no doubt all lighthouses would have been provided with the new light if the question of expense had not stood in the way. It has been stated that the electric light is seen at least five miles farther than the oil-light, and that in hazy or foggy weather the range of the light is twice as great with the former as with the latter.

In England, official opinion was at first against the introduction of the electric light in lighthouses, on account of the peril of interruption; but this has been overcome. There are now electric lighthouses, besides those of England and France, in Russia, Austria, Sweden, and Egypt. Everywhere their action is pronounced satisfactory. Hitherto machines of only 200 Carcel burners have been tried; but it is stated on good authority, that the French Administration of Lighthouses are about to experi-

ment with a Gramme machine of 2,000 burners. This machine, probably, will greatly enhance the advantages, already recognized, of electricity over oil, and will, perhaps, determine a radical change in the existing illumination of light-houses.

The lighting of works by night is highly interesting. The Spanish Northern Railway, after trial, used the light as early as 1862 in the works proceeding in the Guadarama mountains. The expense per hour for material consumed was 2.90 francs per lamp; the saving effected upon the use of torches was 60 per cent. The light has also rendered important services in the mines of Guadarama. The air became so vitiated in the workings by the explosions of charges and the combustion of the miners' lamps that the ordinary lamp would not burn. When a Serrin's regulator was sent down, a complete change took place, respiration "becoming as easy as in the open air," the lamps remaining alight. Amongst open-air works may also be cited, as executed by the aid of the electric light, those of Fort Chavagnac at Cherbourg, of the Chemin de Fer du Midi, the reservoirs of Ménilmontant, the building for the *Moniteur Universel*, and, more recently, those of Havre harbor and docks, the Exhibition of 1878 in the Trocadéro, the Avenue de l'Opéra, the Grands Magasins du Louvre, and other establishments.

With respect to the introduction of the electric light on board ships, the experiments carried on board *L'Amérique*, one of the steamers of the Compagnie Générale Transatlantique, were so conclusive that it seemed as if nothing could oppose the immediate adoption of the electric light on all vessels, for it is satisfactorily proved that the chief number of collisions result from the difficulty which captains experience in estimating the exact position of an approaching vessel. Yet it will be only gradually that the electric light will take possession also of the ocean. Quite recently, the electric light has been introduced into the British Navy. The electric lighting apparatus of Messrs. Wilde & Co., of Manchester, which has been tried on board the Alexandria, Temeraire, and other vessels, having proved efficient, the order has been given by the Admiralty to supply similar apparatus to the ironclads Northumberland, Warrior, Repulse, Superb, Nelson, the steel aviso Iris, and the despatch boat Lively. For at least two years the introduction of the Gramme machine has been progressing on board the war vessels of the French, Danish, Russian, and Spanish navies. As far as we know, up to the present time, no disappointment has been experienced, and increasing labor will doubtless improve the first arrangements and lead to still better means.

As far as the application of the electric light to military operations is concerned, several Governments have ordered powerful Gramme machines for the defence of fortified places. An apparatus specially constructed for this purpose has been adopted in France, Russia, and Norway. From trials made at Mont Valérien with the apparatus, and a special projector, it resulted that an observer at the side of the apparatus could see objects 6,500 yards distant, and clearly distinguish details of construction at 5,500 yards. M. Gramme has also designed a machine for war-signaling of very small dimensions that may be turned by hand. The French army possesses at present two of these machines.

Very little attention has hitherto been bestowed upon the great service electricity will ultimately render in lighting up theatres and similar places of places resort. Besides the comparative cheapness of the electric light, its use will do away with the expensive fitting-up necessitated by gas. The great drawback to thorough enjoyment caused by the flare and heat of hundreds of gas-flames will be entirely removed. No longer broiled and heated up to almost fever point, we shall be able to sit in comfort and, more than that, perfect safety against that most awful of all calamities, a fire in a theater, or even a panic such as quite recently occurred at Liverpool. Panics will be avoided; for people will soon come to know that fire from electric light is impossible. We were forcibly reminded of the great danger to which audiences are now exposed during a recent visit, on a Saturday evening, to the Covent Garden promenade concerts. We pictured to ourselves the scene that would ensue if, during one of those

crowded performances, the mass of inflammable material which has been piled up, in addition to what is already stored there—with the evident endeavor to "decorate" the place—were to catch fire; it required but a little fancy to conjure up a picture of an Inferno to which nothing was wanting. With the electric light, on the contrary, we should have, instead of a sweltering, gasping multitude, an audience able to enjoy the musical or dramatic fare set before it. As yet, however, little progress has been made in the employment of the electric light inside theaters.

What is said of theaters applies with even greater force to places of public worship. Let any one who doubts this attend on a sultry evening at such popular churches as the Metropolitan Tabernacle, or at Mr. Newman Hall's church, or even the City Temple, where the ventilation is said to have been recently improved. The heat in all those places is simply unbearable; yet people are expected to worship under such conditions.

For these reasons, it seems to us, that the reform pointed out will not be long before it is introduced, especially as more perfect machines for producing the light are to be had. In Paris, experiments have recently been made for lighting up the interior of the Opera, with a view of preserving the paintings with which the walls of the *foyer* are covered. The general opinion expressed was favorable to its employment.—*The Builder.*

Of the present state of progress in this country, the *Tribune*, of Nov. 16th, says:

The situation in regard to the electric light at this time resembles in a marked manner the state of things when engineers were trying to make a ship go by steam power. For forty years after the close of the Revolutionary War in America, hundreds of ingenious minds were busily at work trying to solve the problem of the application of steam power to propulsion. The United States, England and France were foremost in the study of the matter, as they are at present in the field of electric lighting. It is not extravagant to say that a thousand forms of banks of oars, paddle-wheels, screw propellers, power pumps, and other devices for making a boat go, were invented, cast aside, taken up again and again, re-invented by men who were not aware of what had been already thought of and pondered over, during that period of forty years, until out of the whole vast throng of crude notions the world finally decided upon two forms of propelling apparatus as practical, and put them into general use throughout the field of marine engineering. Exactly the same thing is going on now with reference to electric lighting. Since 1845, when the voltaic arc emerged from the laboratory as a thing which could possibly be put to practical use, the problem of the electric light has enlisted the attention of a large number of able men in Europe and America. Experiments have been making in hundreds of laboratories. Valuable ideas have been occasionally hit upon and patented, and every ten years or so, the announcement has been made that the means for subdividing the electric light has been found, and the attention of the world has been riveted for a while upon some new lamp which has run a brief course of popularity, and then has dropped out of sight never to be heard of again, until some new inventor has come upon the same idea and brought it out afresh, believing it to be new. No doubt, out of this busy investigation will come eventually a practicable method of sub-dividing the electric light for domestic uses, just as success attended the study of paddle-wheel and screw propulsion. Practical modes of electric lighting on a large scale have already been found. In that direction further time seems to be necessary only for perfecting the details. Lighting on a small scale is yet a thing of the future, unless Edison and a few others have attained to it, but it can hardly be doubted that success will eventually be reached.

Rumor asserts that Mr. Edison's plan is the revival and improvement of a very old one; that of rendering platinum wires incandescent on each of the many branches of his divided circuit. His invention it would seem, however, has not yet passed the experimental stage.

Another light which claims attention is, the so called Sawyer-Mann light. It is produced by the incandescence of a tiny perpendicular bar of carbon, an inch long and $\frac{1}{10}$ inch in diameter, which is sustained in place by two large thick bars, arranged one above the other hori-

zontally. The current, in passing from one large carbon bar to the other, through the small one, encounters great resistance. It heats the small bar to whiteness, and produces a light of the most admirable character. It is white, mellow, and pleasing to the eye, and floods a room with a radiance resembling that of daylight. The blaze in the Sawyer lamp can be turned up and down just like a gaslight. In order to prevent the combustion of the carbon, which would be almost instantaneous in the open air, the lamp is enclosed in a sealed glass tube, two inches in diameter and six or eight high, from which the atmospheric air has been expelled, and into which nitrogen or some similar gas has been introduced. Mr. Sawyer refuses to state at present exactly what the composition of this gas is. He claims simply that it will preserve the carbon from combustion for an indefinite length of time. The lamps are very handsome. Five of these lights are now on exhibition at the shop of the inventor. Three of them are operated each by a branch from the main wire. The two others are worked by one branch.

The Rapieff light now undergoing trial in London, is of another kind. The voltaic arc is produced in this lamp between four carbon rods, arranged in pairs, each pair forming the letter V. The apices of the Vs meet in a common center. A regulator is attached, which maintains the carbons at an invariable distance, and results in a light as regular as that of any other similar lamp. One of the carbons of this patent can be removed and replaced without interrupting the current, which is a new feature. Three lights are maintained in one circuit.

The light in the Brush lamp is produced by the voltaic arc between two carbon points, which are kept at a proper distance from each other by a special device of the inventor. When more than one lamp is used in a shop, the light is practically as steady as that of gas. The lamp is hidden in an opalesque globe which softens and diffuses its radiance. The carbons last for fourteen hours. The light costs one-fifth the expense of gas. A dispatch from Cleveland says, that a new Brush machine now maintains seventeen strong lights.

In the Wallace lamp the light is produced between the edges of two carbon plates, which will last one hundred hours. The passage of the current consumes the edges of the two plates at the point where they are the nearest together, and then flies off along the crack that separates them to some other point, always appearing at the place where the plates are the closest together. The flitting of the voltaic are back and forth causes the light to flicker, and seems to exclude the lamp from domestic use. The furnishing of a shop with a number of them results in a partial correction of the evil of flickering. As far as the light which falls upon the work which is going on below is concerned, it seems sufficiently steady for practical use.

The Arnoux & Hochhausen light, and the Weston light, which is on exhibition at the Equitable building, both employ the voltaic arc between carbon points.

In the Lontin lamp a slender rod of carbon is kept in contact with a slowly revolving wheel, touching it on the outer rim; the small rod is kept for about an inch of its length incandescent.

Of the Werdermann lamp we take the following account from English sources.*

This gentleman uses carbon electrodes, with a peculiarity in their use, which is of some importance. A long slender rod of carbon is secured in a vertical position, and is pointed at the upper extremity, which impinges against the under side of a block of carbon made in the form of a thick disc. Thus one electrode is a pointed rod, while the other presents a flat surface. In regard to the vertical rod the upper part only is in the circuit, and the length of this portion can be increased or diminished by shifting the collar which transmits the current. The rod is drawn up so as to press its point against the under surface of the disc by means of a fine cord and a counter weight. As the rod burns or wastes away, it continues to be drawn up so as to remain in contact with the disc. When the current passes, that portion of the carbon rod which is above the connecting collar becomes incandescent, and an infinitesimally small electric arc is also formed at the spot where the point of the rod touches the flat surface above it.

* *Iron.*

By this arrangement the waste of the upper electrode is exceedingly slow, and is imperceptible unless extending over a period of at least some few days. The rod retains its pointed form during the whole period of its combustion, and may be several feet in length. At a recent trial the current was derived from a small Gramme electro-plating machine, requiring a steam-engine of only 2 horse-power to put it in full work. It may, therefore, be assumed that this was about the limit of the power at work to produce the light. At the commencement of the proceedings two lights were maintained, each stated to be equal to three hundred and twenty sperm candles. At this rate the two lights would be equal to six hundred and forty candles, or forty full power gas lights, each consuming 5 cubic feet of sixteen-candle gas per hour. The two lights burned with extreme steadiness, there being no undulation or flickering whatever, although there was no glass globe to tone down any variations of luster. The lights were perfectly bare and unprotected, and the place where the trial was made was a workshop of moderate size. Later in the evening, one light was exhibited outside the building, in an open thoroughfare, and the same perfect steadiness was observable. After the two lights had been burning for a time they were extinguished, and the current was sent through a row of ten lamps. The light per lamp was of course reduced, but there was the remarkable fact that ten lights were maintained by a comparatively weak machine, driven by an engine with a power of only two horses. The light of each of these ten lamps was stated to be that of forty candles, making, therefore, a total of 400. A reduction of light, consequent on the further division of the current, is thus apparent; but for this loss there may be ample compensation in the economy of a distributed light as compared with one that is concentrated. In the case of the ten lamps, the light is equal to that of twenty-five full power gas lights, consuming altogether one hundred and twenty-five cubic feet of gas per hour. The extremely small arc due to the peculiar arrangement of the carbons in the Werderman light has the advantage of offering the least possible resistance to the passage of the current. Hence the electric power is economized, and it becomes possible to make use of an electric current large in quantity but of low intensity. The tension being small, there is the less difficulty with regard to insulation.

Concerning the transmission of the current, it may be remarked that there are two main wires connected with cross wires which pass through the lamps. If one lamp or more should be accidentally extinguished, the rest will continue to burn. The whole of the lamps can also be extinguished and relit by merely stopping the current and then sending it on again. No nice and troublesome adjustment with reference to the length of the electric arc is requisite. The lower carbon, drawn by the line attached to the weight, travels up through the collar or ring which connects it with the circuit, and simple contact between the point of the rod and the surface of the disc is sufficient for the manifestation of the light. Mr. Werdermann asserts his ability to distribute the current from the small machine so as to divide it among sixty lights. In that case, the light would inevitably be small; but enough is apparent to prove that so far as a current can bear division, Mr. Werdermann will be able to utilize it. In respect to duration, a carbon rod four millimeters in diameter and a yard long, obtained from Paris, costs a franc. This, placed in the large lamp, having an estimated lighting power of 320 candles, will last from twelve to fifteen hours. The smaller lamps take a carbon of three millimeters in diameter.

Of the success from an economical point of view of electric lighting the accounts are somewhat conflicting. The following is the abstract of a report of Mr. Stayton the surveyor for Chelsea, who inspected the Paris system:

The Municipality of Paris have contracted with the general Electricity Company to light up certain streets and places, of which the principal are the Avenue de l'Opera and the Place de l' Opera, a street 900 yards long and 30 yards wide. To do this 46 columns have been set up at an average distance of 38 yards apart. Jablochkoff "candles" are used, and Gramme machines. There are three machines, each supplying sixteen lamps, and each driven by a 16-horse

power steam-engine, set up for the purpose, so that every lamp takes one-horse power. The conducting wires are laid in the subways below the road. The "candles" last an hour and a half, and cost 7½d. each. The number of candles required for the evening are placed in what is termed a "chandelier," within a ground glass globe, a fresh candle being automically switched in as the previous one is consumed. Each light is equivalent to 700 wax candles, but the globes take off one-third of this light. The ordinary London street lamp is equal to 12 or 15 candles.

The contract is at present merely an experimental one. The company undertook to light the lamps for a period of six months, ending in November next, from dusk till shortly after midnight, and to provide the whole of the apparatus, for 1f. 45c. (1s. 2½d,) per light per hour. Shortly before the electric light is extinguished, about one-third of the gas lamps are lighted, and continue till sunrise, the former light being unnecessarily powerful, and too expensive to be maintained all night.

The Avenue and Place de l'Opera are usually lighted by the large number of 400 gas lamps, set three or five together on columns, with short intervals between them. In spite of the amount of gas burnt, the City Engineer says that "the cost of the electric light is four times that of gas, but a greater amount of light is obtained." On the other hand, in the lighting of the courtyard of the Louvre, it is asserted that a saving of 29⅔ per cent. is effected by replacing 201 gas lamps by 16 electric lights, although 3½ times the amount of light is given.

Besides the place above mentioned, the electric light has also been adopted for lighting the Place du Théâtre Frangaise, the Madeleine, the Arc de Triomphe, the Orangerie des Tuilleries, the Magasins du Louvre, and about thirteen other places in Paris. It is also in operation in the principal places in Brussels, Madrid and St. Petersburg.

Mr. Stayton then proceeds to consider the cost of lighting various parts of Chelsea by means of electricity, as compared with that of gas lighting. To begin with, there is less gas used here than in Paris. The distance between the lamps varies a good deal, for instance, it averages 55 yards in Sloane street, 70 yards in King's road, 35 yards in Lowndes square, 35 yards in Cadogan place, 28 yards on the Chelsea Embankment. In Piccadilly the distance is 30 yards, and in Cromwell road, South Kensington, 27 yards.

To light Sloane street, which is 1,100 yards long and 20 yards wide, with two electric stations, each supplying sixteen lamps, would cost for plant and alterations £3,200, and 16s. per hour for 3,250 hours, or £2,600 per annum. The present cost of a gas lamp in Chelsea burning 3,850 hours per annum is £3 6s. 7d., therefore the expense of the 40 lamps in Sloane street is 8¼d. per hour, the total per annum for the street being £133 3s. 4d. The outlay for lighting the Chelsea Embankment with 48 lights in place of the 109 gas lamps is estimated at £4,800, and the hourly cost at £1 4s. for 3,250 hours per annum. The present cost of the gas lamps is 2s. 1½d. per hour for 3,850 hours per annum. In Sloane street the light would be 31 times as great as at present, and it is believed that half the above number of lights would be sufficient. These, however, could not be worked from a single station, so that the chief cost, that of the machines, &c., could not be thus saved.

The main conclusions Mr. Stayton draws are: That the present arrangements for electric lighting are unsuitable for long distances, especially in London, where the lamps are so much farther than in Paris. The close proximity of the electric stations is a great drawback to the system, and their establishment in business streets would be a matter of considerable difficulty. These are the disadvantages of the system. The following are the advantages:

About 1½ hours' daily consumption is saved in consequence of instantaneous lighting and extinguishing; the light is vastly superior to gas, and is not injurious; there is an absence of noxious smells both in the production and combustion; the heat in a room, so often unbearable in the case of gas, is scarcely felt; the most delicate colors are preserved; air is not consumed as in the case of gas; there is no chance whatever of explosion; and although the light is so powerful in the streets, no accidents to horses have occurred.

On these grounds, he says that after a careful consideration of the whole question, he is of opinion "that at present the electric light is not suitable for street lighting in the metropolis; that it is suitable and can be utilized with splendid effect in large squares and places, such as Trafalgar square or Parliament square; but although in each of these places at the present time the lamps are numerous, the cost would be greater than gas." He also makes some remarks on the improvements and modifications required before electric lighting can come widely into use, such as the necessity for further subdivision, and for a means of working over greater distance, points which are of course familiar to those who have paid any attention to the subject.—*Journal of the Society of Arts.*

Regarding the matter of efficiency and economy of the dynamo-electric machines, Mr. Sprague,* the electrician, says:—These machines are manifold in name and in appearance, but they all are different modes of obtaining the desired transfer of energy by the same fundamental principles; the whole object of the difierent inventors is to discover how to arrange the parts so as to do the work at the least first cost and at the smallest working expense. The whole question among them is, *For a given horse-power of engine which machine will produce the largest electric current in a given external resistance?* That question is much too large a one to even enter upon, and it is a good way from being finally settled as yet.

Batteries produce a constant current always in one direction; machines generally produce a constantly breaking and reversing current; but the Gramme machine succeeded in producing a current exactly resembling that of batteries. It is a very debated question which kind of current is best adapted to electric lighting. The positive carbon, which is usually the uppermost, consumes much more rapidly than the lower, and also forms a hollow crater which encloses a large part of the light; the intermittent alternating current generates a resistance in the arc which diminishes the light. However, the Jablochkoff candle, in its most advanced form, requires this alternating current in which the two carbons burn away equally, and at the same time the insulating material is rendered incandescent and contributes to the light; it being, however, not a little doubtful whether this contribution is fairly equivalent to the share of the electric current thus appropriated and taken from the true arc. I merely indicate these points as practical questions by no means solved as yet, but which cannot be discussed here.

As one consequence of this property of the candle, to enable the Gramme machine to be used a second machine has had to be devised, in which the continuous direct current of the Gramme proper is converted into an alternating one. This second machine, however, at the same time raises the power of the current in the same manner as was done in the machines of Wilde, the origin of all the later forms.

The other machine in use in Paris is that of Lontin, which is the one employed also at the Gaiety Theatre in London, and to which public exhibition of the electric light in a supposed practical application is due much of the excitement on the subject. This machine, likewise, is in two separate parts, so arranged as to provide several distinct circuits to work separate lights. Serrin's regulator is used, but M. Lontin claims to have effected some improvements therein. We may now proceed to the applications made of these two systems, and no doubt some of my remarks may excite surprise, because there has been an immense deal of exaggeration and nonsense published by people who, led away by first impressions, and having only partial knowledge of the subject, have, as is usually the case with unskilled observers, seen imperfectly and recorded impressions rather than judgments.

There are at present displayed in Paris a very great number of electric lights, but they may be studied in a very simple set of groups based upon the purposes for which they are used; this also happens to properly distribute the different systems in operation.

1. *Railway Stations.*—At the station of the Western Railway (Gare St. Lazare), Lontin's system is in operation.

Two lamps are placed at the principal entrance; five lamps are placed in the

* Electric Lighting, its State and Progress, by John T. Sprague.

principal hall, where they replace thirty-four gaslights, and will no doubt do so to very great advantage as far as light is concerned, as the hall is so large that the gas has little power over it. They will all be worked by one apparatus, and were to be in action in a few days; when I saw them the engine was not ready.

The goods department is, however, lighted by six naked electric lights, derived from one machine driven by a steam engine. The result is extremely effective, as the men loading and unloading can see the addresses on the packages with ease, and the lights being distributed around the area of operations (which is, however, only a small one), they are able to work easily and safely.

2. *Hotels and Shops.*—In the large buildings opposite the Louvre, which include a great hotel and an immense series of rooms, forming the "Magasins du Louvre," where an endless variety of goods are displayed, there are a large number of electric lights employed which belong to the system of the Jablochkoff candle, as do all the others I shall notice. The courtyard of the hotel was formerly lighted by 14 two-light gas lanterns, and by 24 single globes upon the staircase which forms one side of the roofed-in quadrangle. It is now lighted by 2 electric lanterns on one side, and 6 globes spread over the staircase; 2 other lights in the entrance also assist the effect. In the show-rooms there are a number of electric lights, and also a number of 6-light gas chandeliers, which latter quite equal the electric candle, except as to the inherent difference of the lights as to color, one of the special advantages of the electric light being its possession of all the colors forming perfect light—so that, unlike gas, it shows up clearly the blues and greens, and all the various shades of color; of course, also, the electric light is less heating, does not consume the air, and does not distribute vapors which can condense among the goods.

It will be seen that in this courtyard 52 gas burners, giving, at say 10 candles, a light of 520 standard candles, did formerly the work upon which at least 8 electric lights are now employed, giving, at 300 candles, a light which should be 2,400 candles, and this being nearly five times the quantity of light, the effect ought to be great. In fact, this courtyard is really one of the most beautiful results of the electric lighting that I have seen. I took two friends to see it, and they expressed the same opinion, and thence arose a somewhat curious circumstance. Before leaving Paris they said, "Let us go and have another look at the Hôtel du Louvre." They went, and said as before, "Well, it is really beautiful." "It is beuutiful," repeated one, "but *look again.*" They looked again—the *electric light was not in action; it was the gas light they were admiring.* This shows how large a part imagination plays in many observations.

In this instance I propose to make some remarks here upon the question of cost.

It is stated that here "201 gas burners, costing 90.4 francs per day, have been replaced by 16 electric lights, costing only 63.6 francs, being an economy of 30 per cent., with three-and-a-half times the light." This statement is made by those interested in the light, and I understand that the electric system is worked by an already existing engine, and do not know if a proper charge is included for the steam engine, and for depreciation and interest on the outlay. But *in Paris the gas costs 6s. 9d. per 1,000 feet*—more than double the London price—and its illuminating power is lower than ours—a statement which will surprise those who, seeing the brilliant lighting of Paris, overlook the reckless consumption of gas to which this result is due. The most favorable result, according to English prices, is that, in this case, the actual electric lighting cost 35 per cent. more than the previous gas lighting.

3. *The Streets.*—The Jablochkoff candle is used in many places, chiefly in front of fine public buildings, which they illuminate very effectively, but it will be sufficient to deal with the principal exhibition in the Place and Avenue de l'Opéra, a roadway which extends some 900 yards by 30 wide, and offers from the present aspect one of the most beautiful and interesting sights of Paris.

In front of the Opéra there are two columns, each carrying 3 candles in globes. In the Place are four columns, carrying lanterns, each containing 2 lighted candles. In the Avenue there

are 36 candles irregularly distributed, but averaging nearly 40 yards apart. In the square in front of the Théâtre Français there are two circles of 6 lights each and two single lamps. There are thus in one view 64 electric lights. Besides this there are innumerable gaslights in the shops, and although by the effects of contrast the gas lights seem to be nearly extinguished by the electric ones, they none the less supply their quota of light.

It is needless to say that, regarded simply as a sight, the effect is very striking; it is not at all surprising that those who look at it with the eye, unguided by the judgment, and compare it with our London streets, exclaim about its perfection, and abuse our London gas. By-and-bye I will count the cost; at present I will only indicate some facts which no one has mentioned as yet, and which will somewhat surprise both the casual observer who goes away exclaiming that such a light must be introduced everywhere, and also those who think only of the vivid character of the electric light itself.

(1) It will naturally be supposed that two electric lights placed only forty yards apart will illuminate *perfectly* the intervening space; on the contrary, a distinct gradation of light approaching to a comparative shade, is observable on the roadway, just as with our gaslights. This is, however, a natural consequence of the fundamental principle of light and of all radiant forces that their quantitative effects diminish in the ratio of the squares of the distances; for this reason, where equally diffused light is required, a number of small lights are necessarily more effective than a much more powerful light concentrated in one or few centers. In this case, with the two lights at forty yards, the combined lights give at the middle point of twenty yards a light somewhat less than half of what they give at the ten yards distance from either post; the exact relative values being as $\frac{1}{2}$ is to $1\frac{1}{16}$, calling the value of each light singly 1 at ten yards distance.

(2) People talk about the gaslights in the streets superseding the shop lights. On the contrary, not one single shop light has been put out, nor has the light the least effect upon the shops. I spoke to one jeweler (besides several other shopkeepers) whose front was directly under a candle, distant only five yards. He still used the ordinary outside lamps, and when I asked if he found any advantage from the electric light, he replied, with the usual French shrug, "Why, sir, it is simply moonlight." This is, in fact, the exact description of the illumination; it is that of a moderately clear moonlight with the eye caught by points of brilliant light, and, so far as the shops are concerned, this light is absolutely ineffective. We often complain that our gaslights are largely wasted upon the heavens; most of the beauty of this Paris light is really due to that cause, the long rows of lofty white houses catch the eye and also reflect part of the light back again, and the air itself is filled with a luminous haze due to the reflections from the innumerable minute particles with which it is always loaded.

(3) The light is variable. Each candle is continuously flashing and frequently changing to a rosy tint. This might not be noticed by a casual observer, being lost in the general effect, but on examining the light by writing directly under a lamp, I estimated the fluctuations as amounting to fully one-fourth of the full light.

(4) The light is rapidly lost in the distance. There is a row of lights in face of the house of the Legislative Assembly which light up the façade very effectively. In the Place de la Concorde these lights stand vividly out from the surrounding gas lamps; at the front of the Madeleine, double the distance, they are only distinguishable from the ordinary street lamps by their superior whiteness of color, the Paris gas being very yellow. This observation will no doubt seem incredible to many; it appeared so remarkable to me that I have worked out some calculations which prove that my sight and judgment told me the strict truth, as will be seen in a subsequent paragraph.

4. *Theatres.*—The Hippodrome, a very large circus, is now lighted with the Jablochkoff candles. There are twenty in globes along the line which divides the audience from the arena, besides three gaslights (or sixty burners turned towards the audience) on the intermediate columns. Above these again are sixteen naked candles with reflectors throwing the light on the middle of the arena.

There are thus thirty-six electric lights in full action, and in addition, there are four naked lights suspended from the roof to throw light upon the Trapeze performances when required. But with all these powerful lights the result was poor compared with the rich radiance we are accustomed to in theatres. As in the streets, it was an effect similar to moonlight, and the fluctuations were very perceptible. I was myself greatly surprised at this, as I was prepared to find the light especially well suited to this purpose, and I can only explain the actual fact by supposing that the eyes are dazzled by the number of cross lights directly influencing them, instead of illuminating only the objects intended to be seen.

II. The Cost of the Light.—As to this, it is nearly impossible to attain any reliable information. We can only accept at a suitable discount the statements furnished by the interested parties.

The Gramme machines, to maintain 16 lights, cost £400 in addition to a steam engine or other motor of at least 20 horse-power. The fittings of the lamp cost £8, besides columns, &c., and the conducting cables 3*s.* 4*d.* per yard of the circuit. These are Paris prices, and would be higher in England. Smaller machines for fewer lights would cost more in proportion.

The cost of working per hour for the carbon candles, firing of engine, and attendance, is put at 8*s.* 8*d.* per machine, or 6½*d.* per light: to this must be added a due charge for repairs, depreciation, and interest upon outlay, so that the hourly cost cannot be put at less than 8*d.* per light. But any calculation on this basis, without taking into consideration the circumstances of each case, would be so uncertain that I shall here confine myself to the one salient fact agreed to by all the interested parties, Gramme and Lontin in France, and Siemens in England, that *each separate light consumes one horse-power of an engine*, besides all the incidental expenses.

I have not the prices of the Lontin system, but I believe they are somewhat less, but I know that the Lontin light, as displayed at the Gaiety, tries to the utmost the powers of a 12-horse engine. I would not propose omnibus drivers as judges upon a scientific problem, but I may mention that one of them explained to his passengers a few evenings ago that these lights brought great crowds to look at them, and that it was found they were too strong, so that two of the lamps had to be put out. The man was quite right, they were too strong—*for the engine.*

The quantity of light produced in return for this cost is somewhat uncertain. It is claimed that the regulator with Lontin's system gives a light equal to 100 Carcel lamps, and the Jablochkoff candle about 80. I very much doubt this. I found no difficulty whatever in examining the light steadily at a distance of 12 or 15 feet, nor did it produce any subsequent unpleasant impression of the spectral nature; and I cannot think that this would be the case with a light of the intensity stated, proceeding from an illuminating area not larger than a sixpence. It is, however, claimed as a merit of the Jablochkoff system, that, "instead of a luminous point emitting divergent rays of a most disagreeable character, it bathes the brightness of the carbon points in a white flame which gives a diffusive light," but my remark applies to both lights.

There is some doubt as to the value of the Carcel lamp in our standard candles, but Mr. Sugg, who is a good authority, valued it some years ago at 9.5 candles, and I will take this value as the highest. This gives the light of the "candle" as 760 standard candles: Mr. Sugg, however, states that on a trial before the jury the light of each as tested was 500 candles. These lights for most purposes have to be enclosed in opal glasses about 16 inches diameter, which are said to absorb one-third of the light. It is, however, well known from the best experiments that

Plain glass	absorbs	10	per	cent.
Ground	"	"	30	"	"
Opal	"	"	60	"	"

This reduces the effective lighting power of each light to 300 candles, assuming its original value to be, as claimed, 760. With these globes the light no longer issues from the radiant point, but from the whole surface of the globe, its inherent brightness being therefore proportionately reduced. An ordinary gas

burner giving, say 10 candles light, has an illuminating area of about 5 square inches, so that its inherent light is about 2 candles per square inch. The area of the diameter of a globe of the size used is 220 square inches; and if its light is 300 candles, the inherent light is only about 1.4 per square inch, less than that of the gas burner. Hence it is that when we increase our distance so far that the size of the flame no longer influences the eye, and that its rays are practically parallel, the electric lamp is, as I before stated, distinguishable from a gas lamp only by its different color.

Ordinary gas, burnt properly, will give the light of 14 candles for 5 feet per hour; that is, 1,000 feet give a light equal to 2,800 candles for an hour at a cost of, say 3*s*. 4*d*. Assuming that an electric light gives a net effect of 300 candles, and that its cost is only 8*d*. per hour, the cost of the same 2,800 candles is 6*s*. 2½*d*. With naked lights, however, the cost of gas would be somewhat greater for equal amounts of light. But all such calculations are at present only vague approximations. Improvements also must be looked for, and, indeed, I have seen in operation machines, not yet before the public, which will, I believe, lower the cost. On the other hand, there is abundant room for improvement in the use of gas, as it is well known that even our best burners do not give anything like the light the gas is capable of, while those in common use burn the gas most wastefully. In fact, we burn too little gas in single flames, so that the surrounding air floods and cools the flame; the more gas we can burn perfectly in a single light the greater the light the gas will give for the quantity burnt. A simple experiment will prove this: Fit two burners on pieces of elastic tube so that they burn side by side, and note the light given. Now bend them to each other so that the two flames cross and blend in one; it is evident there is no increased consumption, but the increased illuminating power is something surprising. If powerful lights, such as the electric ones, are required, there is little doubt they can be produced economically from gas, and all the other objections can readily be overcome, for the color of gas light will whiten as the quantity properly burnt is increased, until a pure white light is obtained, superior in quality to that of the electric arc, because the intense temperature of this involves the production of an excess of the violet rays of light and of those chemical rays called actinic, which, while consuming energy, do not give light, and from their other properties are likely enough to have injurious actions upon health. The removal of the products of combustion also is a very simple matter, already effected in some forms of gas burners, and involving nothing more than an outlet to a chimney or to the open air. In fact, all the defects attributed to gas are simply matters of management and economy, and if people really wish to correct them, and are prepared to spend only a small portion of the amount needed for the fitting up of electric lights, they can easily have what they desire, and yet retain that great advantage in simplicity, the mere turning on of a tap, as compared with the difficulties of special and elaborate mechanism.

At present it is found that the current for producing light cannot be sent to any great distance with advantage. Leakage has been the great trouble of gas engineers, but it is a mere trifle in their case as compared with the transmission of large electric currents. What is called *resistance* in electricity is of two orders: there is the conductive resistance, the specific capacity of each substance to permit electricity to pass, and which may to some extent be compared to friction in mechanics. This is a simple enough matter, and its law is that with a given kind of conductor, say copper wire, to maintain a fixed resistance the section of the conductor must increase in the same ratio as the length, or, what is the same thing, the total weight of wire must increase as the square of the length. This alone is simply a question of first cost; but then also the tendency to leakage or loss increases in a greater ratio than the length, and the insulation becomes more and more difficult as the size of the conductor increases. But this is insignificant compared with what is called inductive resistance, that which in submarine cables limits the speed of transmission. This comes into action only with intermittent currents, and it will increase in some ratio exceeding the

square of the distance, if the conductor is enlarged as required to increase the conducting power. We have as yet no experience in this subject with large currents, but it is so serious an obstacle in the minute currents used in telegraphy that it is not unlikely to prove an absolute barrier to the sending of large alternating currents to any considerable distance. I was told that arrangements are making to send a light two miles here in England, and when I pointed out this probability, it seemed that it had not been anticipated, but I learnt at once that it had arisen in some experimental trials. Whether it will be overcome remains to be seen; if not, systems depending on alternating currents, as with the Jablochkoff candle, will be limited to short distance; at present they work at a distance not exceeding 300 yards.

III. Influence upon Gas Interests.—From what has been stated it is evident that the electric light can only be applicable in some circumstances, and that the necessity of cumbersome machinery and qualified attendance must limit its use. But also in many cases where it would be suitable, it by no means follows that its use would involve diminished consumption of gas. An American experimentalist states that with a petroleum engine and electric apparatus he has obtained more light than from the direct combustion of the petroleum. Now gas engines have received of late very great improvements, and these improvements are not final: it is quite certain that from their great simplicity, their freedom from danger, and from the great fact that they require little or no attendance, they will gradually supersede steam for many uses, and among others they would often be employed to furnish the motive power for electric lighting, and more gas would be employed than was required for the former gas illumination, because no one will be contented with merely the same quantity of light.

For street lighting there is small probability of its extensive use. Paris is a great bazaar; abundant light, however great the cost, pays there, just as it does in our public-houses. The people there live in the streets, which are filled with fine public buildings and endless sources of attraction. Neither our climate, nor our cities, nor habits of life, offer us the same inducements; and what would the English ratepayers say if asked to maintain even the ordinary gas lighting system of Paris, where in many streets the public lights are equal to one burner every 4 or 5 yards; such was in fact the light now displaced by the electric light, and still employed in the Rue de la Paix, close at hand and easily comparable, where every 12 or 14 yards there is a post with three lamps on it, giving a light not so white and brilliant, but quite as useful practically, as the electric light itself.

There are a few places such as Charing Cross or Oxford Circus, where a full electric system might be advantageously employed, but its general introduction into English streets is a probability so distant as hardly to call for consideration.

To make this evident I have carefully gone over Trafalgar Square and estimated the electric lights required to illuminate it *exactly as the Place de l' Opéra in Paris is now lighted.* It would require 16 single and 7 double lights, that is 30 electric candles with engines of some 40 horse-power. It is at present lighted by 73 gas burners, not counting private lights.

In lighting railway stations, in large factories where the vitiation of the air by gas is an important consideration, probably in picture galleries and libraries, and in some of the larger hotels, the electric light must be expected to come into use, but its introduction will be gradual, and its utmost effect on gas consumption can only be to somewhat diminish the regular annual increase of consumption. This *might* diminish the need for increasing powers of production, but as the dividends are earned by actual production, these would not be affected, even by a complete stoppage of the increase; it is however much more likely that by the use of gas engines and other employments as yet undeveloped, this increase will grow rather than diminish.

Finally, it needs no prophetic power to foresee that ere long a dead set will be made in the markets both upon gas shares and upon the pockets of that confiding public who are always ready to give their money to any one who asks boldly and promises freely. It is but a few years since gas shareholders went

through a similar scare over new gas, air gas, and other inventions which were to ruin them and make the fortunes of people who gave £20 for £1 shares in the new scheme. The day some bold financier asks for a million of money to light England with electricity, no doubt timid shareholders will rush about to sell their shares, and it will be useless to hint to the speculative investors that the new lighting, when introduced, will probably not be powerful enough to show them their money back again. Gas shares would no doubt go down, and that would be an excellent opportunity for prudent men to invest in gas shares. In fact, after a thorough examination of the matter, so far as it has yet progressed, and so far as its further unquestionable progress can at present be foreseen, I feel no hesitation in expressing the conviction that the gas companies, at all events of London, have nothing whatever to fear, and that whatever owing to market operations may happen to gas *shares*, gas *dividends* are in little danger from any competition with the electric light."

In the meantime each and all of the various systems are on trial in this country. The American machines are the Wallace, the Brush, the Weston, the Arnoud-Hockhausen, and the Fuller—all dynamo-electric machines, converting by slightly different arrangements of permanent and electro-magnets, mechanical power into electric currents.

The lights are of two classes: 1st, those which consist of a luminous arc between two slightly separated carbon points; and 2d, those which consist of a slender rod of carbon or platinum, rendered incandescent by the constant passage of a strong current through it.

In the former class, if the carbon points approach each other from opposite directions, an automatic regulator is necessary to maintain the light; for such purpose are the Serrin, the Browning and the Focault regulators; the Jablochkoff light, although depending on the arc, requires no regulator, because the carbons are in fixed relative positions parallel to each other and at proper distance.

Of the second class, the "incandescent" lights; the Sawyer-Man; the Lontin; the Regnier; the Werderman and rumor says the Edison are the present examples.

It would seem that we are yet without sufficient knowledge of the relative economy of either the machines or the lights, but the public may await with patience the results of the very brisk competition which will certainly be induced by the present urgent demand for improved systems of lighting.

CONDITIONS OF MAXIMUM MAGNETIZATION OF ELECTRO-MAGNETS.

By T. DU MONCEL.

From "Comptes Rendus de l'Académie des Sciences," Abstracts published by the Institution of Civil Engineers.

In former papers the maxima conditions of electro-magnets have been deduced from formulæ in which it has been supposed that the attractive forces were proportional to the squares of the intensities of the currents, to the squares of the number of turns or convolutions of the magnetizing helix, to the diameter of the iron core, and to the square roots of their lengths. The Author doubts that these laws are applicable under all degrees of magnetization, and he has already pointed out* considerable departures within certain limits as regards the diameter and length of core. He now finds this also the case with the law that represents the electro-magnetic forces as proportional to the squares of the intensities of the current. It has long been known that Joule, de Haldat, Müller, and Robinson have found that at the commencement of a current, and when the magnetic state of the iron is far removed from the point of saturation, the attractive force, instead of increasing as the square of the intensity of the current, increases in a much more rapid ratio, and that this ratio again diminishes near the point of saturation,

* Comtes Rendus, vol. lxxxv., pp. 377, 466, 481, 497 and 652.

remains for a short period stationary at this limit, and subsequently diminishes to that of simple proportionality to the current intensities. The question then arises, what should be the resistance of the magnetizing helix with regard to the exterior circuit? The helices, the author concludes, should always have less resistance than the exterior circuit, in the amount of half where g is taken as variable, and in the ratio of $2a+\frac{c}{a}$ to $a+c$ in the case where the variable is a. It is then to be concluded that on circuits where the interruptions of the current are multiplied, the resistance of the electro-magnets should be proportionally greater as the completions of the circuit are of shorter duration; and it is for this reason, as well as for faulty insulation, that Mr. Hughes has considerably reduced the resistance of electro-magnets employed on long circuits. On a line of 310½ miles, Mr. Hughes has found that the electro-magnets of his instrument should not have a resistance above that of 74½ miles of the line. Of course the imperfections of insulation conduce also to this diminution.

THE PROGRAMME OF ENGINEERING AND OF ARCHITECTURAL STUDENTS.

From "The Builder."

We have regarded the main divisions of the professional study of the engineer as far as concerns, first, survey; secondly, public works proper; and thirdly, the application of these motor-powers which nature herself offers to the service of man. The point thus reached may, indeed, be considered as a sort of landmark. The discovery, by Watt, of a means of applying for the production of motion the expansive power of steam, forms an epoch in the history of engineering. The mechanical engineer is sometimes regarded as if he occupied a lower position in his profession than his civil brother. No view can be more inaccurate. The civil engineer must be, to attain any eminence, a mechanist and a workman also. Our first engineers have been mechanics. The minute details of the locomotive have received as much and as enlightened attention from Robert Stephenson and from Isambard Kingdom Brunel as they have done from Fairbairn, or from any exclusively mechanical man.

The use of steam-power, first for the drainage of mines, then for the general movement of machinery, and thirdly for propulsion by movable engines, both by land and by sea, had attained a very important development before the theory of mechanical motor-power was at all properly understood. It is forty-five years since Ericcson, by his successful construction of a hot-air, or caloric, engine, gave a practical proof that the value of the vapor of water for mechanical purposes was due to its capacity for heat; and that heat itself was the source of motion. The determination of the thermal unit was a step of extreme importance in the advance of mechanical science, and the researches which are now being daily pressed further and further into the study of electricity, point to a possible improvement in our methods of applying the force of nature, as much in advance of our present mechanical knowledge as that is superior to the science of a hundred years since. It is true that the cost at which a certain quantity of heat can be produced by the combustion of coal is very far less than that attainable by any other known process of chemical decomposition or recomposition. That fact has been a barrier to the use of electro-motar machinery. But the discovery of the conversion of motion into the electric current, as in the case of the magneto-electric machines of Gramme, is one of which it is impossible to foresee the ultimate results. We can command, in the tidal movement of the sea, a source of motive power that is practically unlimited, as well as gratuitous. The main obstacle to the future

application of this power to the service of civilized life is the difficulty of transmission. The present means of hydraulic and of pneumatic transmission of power are limited, costly, and clumsy, as compared to the use of the insulated wire for the transmission of the electric current. Up to the present time only currents of low tension have been required for telegraphic purposes. But the extraordinary results of the application of a steam-engine of only seven-horse power to the production of the electric light by the Gramme machine, on the one hand, and the discovery of the quadruple or multiple divisibility of the electric current on the other, seem to intimate that we are possibly on the threshold of brilliant discoveries, as to the means of availing ourselves of an unlimited source of motive power now very little employed or regarded.

It is thus, first to the production, and, secondly, to the transmission, of mechanical motor-power, that we hold that the chief attention of the engineer is invited by the actual state of scientific discovery. The study of the accumulator, and of all the appliances of hydraulic transmission of motion, is already assuming a practical importance hardly less that of the study of the steam-engine itself, whether fixed, marine, locomotive; or movable, as in the case of agricultural steam-engines. As to the latter, the chief attention at the moment, especially on the Continent, is turned to the improvement of what are called secondary locomotives, which are required either for the ascent of steep inclined planes, or for the propulsion of vehicles or tramways at comparatively low speeds. It is probable that the advantages thus to be obtained are, at present, rather overrated than otherwise. The economy which is attainable for locomotion over an approximate level, and at a speed not less than twenty miles per hour, is so great, as compared with the former methods of transport, that a saving of four-fifths in the cost is estimated to have been obtained. But with the introduction of the counteracting power of gravity, in steep ascents, and with the reduction of speed, the great economical advantages tend to disappear. We may find a steam-engine cheaper than a pair of bullocks to take a given load up a given hill. But it will be the persons who have to ascend the hill who will have, as a rule, to provide and apply the engine-power. There will not be even that return for the expenditure of the capital of independent persons, who merely seek a profit for their money, that is afforded by our railways; and even that is now only 4.32 per cent.

After the general study of the engineer with regard to the apparatus for the production, and for the transmission, of mechanical motor-power, comes the wide field for his skill in the application of such power. Here we enter on mechanism proper—industrial mechanism, the substitution of mechanical contrivances, exact in their operation, for human skill, which is less exact. The line may here be drawn with unusual precision between mechanism and art—between the mechanic and the artist. It may not be too much to say that we anticipate that almost anything, for the production of which skill may be acquired as matter of routine, will sooner or latter, be better, as well as more cheaply, effected by machinery than by human fingers. On the other hand, in all that into which the real soul and genius of the artist enters —all that which no toil or labor will enable an unsympathetic scholar to acquire —will remain the province of man as distinguished from machinery. In the vast factories now erected for the production of textile fabrics, the operatives are becoming, more and more, the mere attendants on the machines. The saving of labor is now being made in the direction of increasing the number of spindles, of looms, or of other machines, which can be tended by one man. But while a faultless and accurate piece of machine-made lace, for example, is turned out at an incredibly low price per superficial inch, the manufacturer of this fabric does not lower the price of Rose Point or of Honiton. The individuality of the artist, even in so humble a matter as the execution of a delicate pattern in thread, can never be attained by the machine. When what is wanted are great numbers of indistinguishable copies, they will be produced, from year to year, cheaper (and it may be hoped better) by the mechanic. When what is required is to gratify the taste, or to elevate the tone, it will always remain the province of the author.

It is, of course, far beyond our present

limits to attempt to sketch an outline of the application of mechanical power. An instance which may be cited as one of the most extraordinary triumphs of this branch of mechanics, of recent times, is the sewing machine. While for many purposes the work done by this instrument is inferior to hand-work, there is no doubt that the burden of an enormous amount of purely mechanical oil is thus taken off mankind, or rather womankind. The only question is, how far the workwoman has been raised in the social scale by the invention. We very much fear that it is precisely where the heaviest burden formerly fell that it is least diminished, even if changed in the mode of its incidence, by the change. Amongst the most striking of the modern mechanical applications of scientific principles, of comparatively late discovery, may be named the bicycle; and the beautiful American drilling machine, in which one coil of flexible steel wire revolves within another, and the instrument while rapidly working may be applied in any direction. The more wide becomes our acquaintance with the field of mechanical invention, the more profound will be our conviction that the engineer is, in this branch of his calling, but at the very commencement of an immeasurable progress.

Among those special mechanical industries, which at the present moment demand so much of the attention of the engineer, may be mentioned the transmission of signals to a distance by electricity, whether they are designed to attract the eye or the ear; the application of the same power to purposes of illumination, whether by way of instant ignition and extinction of coal gas, or as a direct source of light; the labor-saving machinery of which the farmer is daily making more and more use, and the application of steam locomotive power to what are called secondary purposes—that is to say, to the ascent of steep hills, or to the propulsion of vehicles through the streets of populous towns. In these matters, we see on the one hand the successful application of patient care, and even the flashes of brilliant genius. On the other hand, we have to lament the adoption of suggestions, any one of which may in itself be good, but which, taken together, are mutually exclusive. Such, for example, is to be found in a recent report on local railways, or, as we should call them, tramways, in France, in which the combination of gradient, engine-power, weight of rails, and possible weight of train, is so impracticably proportioned that no train could exceed a gross weight of $7\frac{1}{2}$ tons, exclusive of the engine; while a rate of charge equal to that at present made for horse haulage by the common road would be insufficient to make any return on the capital expended. Here is a case in which the schoolmaster is signally abroad, and in which the need of sound scientific programme is abundantly illustrated.

When we turn from the rough programme of the study proper for the education of the engineer to that appropriate to the architect, we shall see that there is at the same time a close resemblance, and a sensible difference, in the points which it is needful to block out, as compared with those already indicated. As the study of the engineer commences with survey, so does that of the architect. But as the aim of the former is to work in accordance with mechanical law and routine, while that of the latter is to produce the work of the artist, so from the very commencement of their studies should a certain difference of bias be impressed on the respective pursuits of two students. We have regarded survey, as conducted by the engineer, as composed of two branches—that of the making of maps, plans, and diagrams, and that of the inspection of materials and workmanship. This second branch is common to the two professions—the differences, when they exist, being only those of detail. But while the work of the engineer, in the wider branch of science, is mathematical, that of the architect is artistic. Not that the importance of exact and actual measurement of details is to be undervalued. Not that the drawing-board, the T and set squares, the dividers, and the compasses, ought to be less familiar implements for the one pupil than for the other. Not even that we undervalue for the engineer the use of the sketch-book. But his sketch-book, if what it ought to be, will be something very different from that of the architect. A certain degree of proficiency in free-hand drawing, desirable for every one, is indispensable for the student of architecture. Above all, he must be gifted with

that power—rare as it may be—which enables a man to see a picture where it exists; in fact, to appreciate the pictorial in nature. The survey of the architect, in this sense, consists in the filling his mind with the noblest and best specimens of structural beauty and excellence. His memory, no less than his note-book, will be enriched, his taste will be formed, and his judgment enlightened by travel and observation. Not only will he have to view a building in the grand, to appreciate the proportions of its most striking features, and to apprehend the law of architectural subordination, but he will at the same time become aware how far climate and situation are elements of style. He will understand how Egyptian architects burrowed in the live rock for a grateful shade from the burning sun of Africa; how Greek and Italian architects were contented with the artificial shade of peri-style or of vault for the brief heat of the Mediterranean summer; how Teutonic architects throw up pointed and gabled roofs to throw off the burden of the winter snows. As the survey of the engineer gives him exact measurements of space, so the survey of the architect should give him truthful ideas as to situation, adaptation, and proportion.

Distinct from this preparatory schooling and enrichment of the mind is scientific knowledge of structure, both as to mathematical construction and as to strength of materials. Here the architect, for the most part, has to deal with problems of less magnitude than the engineer; and tables and examples will be readily forthcoming to guide his ordinary practice. But the higher and more scientific forms of structure; the groined arch, the flying buttress, the frozen cobwebs of such ceilings as those of King's College Chapel, Henry VII.'s Chapel, St. George's Chapel, with pendentives that seem to mock the unsleeping power of gravity; the heavy dome of a Michelangelo or of a Wren; must contain no secret hidden from his research. Perfect structural skill, founded on mathematical knowledge, is an essential part of the furniture of the mind of the architect.

Hand in hand with this structural skill must rank what, if we retain the original meaning of the Greek word, may be called the economical skill of the architect—his ability in the distribution of the house. This part of his qualification may be described as the education and organization of common sense. It is the portion of education which is most usually and most cruelly neglected, not in one pursuit alone, but in the general upbringing of the young men and boys of England. By the service of this faculty the architect learns, as if instinctively, to make the best of a given site, to proportion means to object, to subordinate parts to the whole, as a matter of distribution of space. All questions of sanitary provision here come in; not only as to drainage, ventilation, illumination, and removal of smoke, but as to due proportion of open area to building, of vestibule and staircase to rooms, of offices and stabling to residences. An architect possessed of this kind of ability will never run a line of street so as to cross a line of thoroughfare and cut off a main street ventilation, as we so often see done in some of the ever-growing suburbs of London. He would as soon make his drains run up hill (as in fact has been done with some of the main drainage of London). Nothing is more easy than to undervalue, or to neglect, this important part of the education of the architect. Nothing is more fatal to the use and the value of his advice.

To these, the mechanical, although far from being the self-moving parts of his art, has to be added yet another element,—the æsthetic. Taste, it is true, cannot be given to him who has it not; but it may be cultivated in him who has it, even if the portion with which he is naturally gifted be but meagre. And even taste may be aided by intelligent perception of structural fitness. For all those features of building which give the finest contrasts of light and shade there is a structural reason. The man who attempts to beautify a building by *appliqué* ornament, stuck on without reason arising from structure, produces only costly abominations. It requires a noble and a cultured taste to make structural lines assume the most pictorial forms, as is the case in so much of our pointed eccleciastical work. But the man who is unaware of his primary principle of decoration is like one who builds sham ruins to decorate a newly-planted shrubbery. The lamp of truth can never be left unlighted by any one who aspires to the name or

the name of an architect, except at the cost of his reputation.

Brief and imperfect as the above sketch admittedly is, it may yet not be altogether without use. The traveler who is encumbered with parcels soon becomes aware of the comfort and advantage that he derives from a list of his *impedimenta*. He knows each package, no doubt, well enough. Yet if they are not numbered or catalogued he will often feel puzzled to tell which is missing in the hurry of a railway station. The value of order, even of the most simple and artificial kind of order, is often incalculable. Were those who direct architectural education to set before themselves the task of blocking out the duties of the profession—of watching against the danger of directing any one, instead of giving attention in proper proportion to all—the buildings now going on all over the country would be of a higher stamp than is too often the case. It is no doubt true, when we look back to the work of architects of a former generation, that much ignorance and neglect of detail often characterize their work. We may see many a building which, viewed at a certain distance, is imposing from its just or even noble proportions, but which yet pains the educated eye, on a closer approach, by its rudeness or even vulgarity of detail. It is quite intelligible that as architects have become aware of this deficiency their efforts to obviate it may have led them to err in the opposite direction. In the careful study of the best details of the old masters, the students have but too naturally lost what is more important than any detail, namely, grasp of the dominant idea. With men of real taste, and real desire to excel, it ought to be sufficient to give this hint. When the architect takes breath—steps back from his drawing-board, as the painter does from the easel—asks himself, "What, after all, was the original motive of this moulding, or of that feature of detail?" he may often start to see how he is forgetting the whole in a too careful study of the parts. The essential value of an exhaustive programme may thus be held to be twofold, both to the architect and to the engineer. First, it should insure exhaustive study, by enumerating every branch of skill and learning proper to the profession. Secondly, it should insure, by the order of its own arrangement, a corresponding order in the mind of the student, and a fit subordination of parts to a complete and perfect whole.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—The last issue of Transactions contains:

No. 143, On a Newly Discovered Relation Between the Tenacity of Metals, and their Resistance to Torsion, by Robt. H. Thurston.

No. 144, Observations on the Stresses Developed in Metallic Bars by Applied Forces, by Theodore Cooper.

No. 145, Cushioning the Unprocting Parts of Steam Engines, by John W. Hill.

Discussions of previous papers by Prof. Wood, Edward Searles, J. Foster Flagg, and others.

Edinburgh and Leith Engineers' Society.—A meeting of this Society was held on Wednesday evening, November 20th, at No. 5 St. Andrew square, Mr. Robert C. Reid, C.E., president, in the chair. A paper was read by Mr. Robert H. Smith, formerly Professor of Engineering in the Imperial University of Tokio, Japan, "On the Calculation of the Strains and Stresses in Redundant Structures." He described a redundant structure as one in which the number of links was greater than $2n-3$, where n means the number of flexible joints, the above-number $2n-3$ being just sufficient to make the structure stiff. He pointed out that what is usually considered a non-redundant structure becomes redundant when laid upon and fixed to its abutments, and that it was on this account that the horizontal components of the abutment reactions were usually said to be indeterminate. He then described in detail the graphic and other methods of calculating these horizontal components from the *moduli* of elasticity of the different members and of the abutments, treating separately, link-work structures, and massive arches, neither of which, he believed, had hitherto been completely investigated. After explaining the application of the general method to structures of any degree of redundancy, he concluded with some remarks upon the impossibility of profitably avoiding all redundancy in engineering structures.

Institution of Civil Engineers.—The Council of the Institution of Civil Engineers are inviting communications on the subjects named below, and for such as are approved they are prepared to award premiums arising out of the special funds bequeathed for the purpose:—(1) The triangulation survey, and mapping of countries and districts, including the astronomical observations required for latitude and longitude, and the measurement of bases, with a description of the instruments employed, the reduction of the observations, and degree of accuracy of the results; (2) The leveling of countries either by spirit-leveling, vertical angles, barometers, or the boiling point

of water; with a description of the instruments employed, the reduction of the observations, and degree of accuracy of results; (3) The effect of the lapse of time on the strength of materials strained beyond the supposed limit of elasticity, but within the ultimate strength; (4) The stresses inducing the failure of iron ships, as bearing on the probable endurance of large iron structures under high strains and repeated bendings; (5) The causes of slips in rocks and earths of different kinds, and the conditions that induce treacherous ground in railway cuttings, tunnels, and the sides of valleys near reservoir banks; (6) The best combined system of warming, ventilating, and lighting large buildings; (7) The most suitable materials for, and the different systems of, road-making for large towns, where the traffic is heavy, including a comparison of first cost, maintenance, and durability; (8) The construction of iron piers for viaducts, with practical examples of French, American and other systems; (9) The design and construction of a steel bridge, with particulars of the weight and cost, and of the tests to which it has been subjected compared with an iron bridge of the same span; (10) The works carried out on the Continent of Europe for the improvement of rivers, and of inland navigation generally; (11) The design and construction of movable weirs across rivers; (12) The treatment of estuaries, with special reference to tidal capacity; (13) The design and construction of dock gates and caissons, including the requisite external and internal arrangements, illustrated by recent practical examples; (14) The design and construction of building slips for large vessels; (15) The construction of tide gauges, and the usual method of carrying out a systematic series of tidal observations; (16) The storage and filtration of water, both natural and artificial, and the arrangements for the distribution of water in towns; (17) The systems of domestic water supply suitable for rainless districts; (18) The benefits and expedients of irrigation in India and in other warm climates, and the proper construction of irrigating canals, so as to avoid erosion or silting, and to prevent the growth of weeds; (19) The bearing of recent experiments on the resistance of vessels, and on skin friction, upon other hydraulic problems; (20) The most recent types of iron sleepers for railways, with practical results and statistics; (21) The best practical use of steam in steam engines, and the effects of the various modes of producing condensation, and of various grades of expansion; (22) Compressed air as a motive power, particularly as applied to machinery in mines, and for traction on tramways and in tunnels; (23) The relative advantages of steam, heated air, gas, water, and electricity as the motive power in small engines; (24) Wind and water as motive powers, compared with steam power, and the motors most suitable for utilizing them; (25) The differences in design of British and foreign locomotive engines; showing the benefits derived from increase in weight, and the relation that ought to exist between the diameter of the wheel and the load it has to carry; (26) The various descriptions of pumps employed for raising water or sewage, and their relative efficiency; (27) The different systems of lifts in use in warehouses and in dwellings; (28) The relative loss of power due to friction in various parts of machinery; (29) The "output" of coal in the United Kingdom, as compared with that of other countries, illustrated by statistics, showing where coal is produced, where and how it is consumed, and the relative quantities exported; (30) The appliances and methods used in different countries for tunnel-driving, rock-boring, and blasting, with details of the cost and of the results attained; (31) The methods and machinery employed in sinking and in working deep coal mines; (32) Coal depots for ocean steamers, the various points involved in their management, and the methods of preserving large quantities of coal from deterioration; (33) The metalliferous or other mining districts in different countries, and the mode adopted in working them; (34) The methods employed in securing the excavations in mining large and irregular-shaped mineral deposits, for example, the Almaden mines, the Great Comstock lode, etc.; (35) The appliances used in different countries for dressing the ores of lead, copper, zinc, and tin, and the smelting of such ores, with details of the results and cost by various methods; (36) The disposal and utilization of slags from various smelting processes; (37) The management of underground waters in mining districts, and the relative economy of distributed or trunk pumping engines, adits, etc., in particular cases; (38) Recent progress in telegraphy, with a notice of the theoretical and practical data on which that progress has been based; (39) The application of electricity to lighting purposes, contrasted with the best systems of lighting at present in use; (40) Torpedoes, and their influence on naval construction.

IRON AND STEEL NOTES.

THE COOLING OF STEEL DURING HARDENING.—One of the most serious losses common to our tool and implement manufactories is that of the cracking and splitting of steel during the hardening process. Not only is the article or piece lost after having incurred the cost of its manufacture, but, in many cases, the completion of the machine of which it forms a part is arrested until the lost piece is replaced. In many cases this is done at increased expense, because the piece has to be made singly instead of with a number of others, involving as much setting of machine and adjustment of tools as would be required for a large number of pieces. Successful hardening and tempering is, indeed, even under ordinary and unvarying conditions, considered and kept as a trade secret. Visitors are excluded from the hardening and tempering room. In some cases the method of heating, in other cases the material used for heating, in yet others the cooling mixtures form the supposed secret. As a matter of fact, however, some of the very best tool manufacturers employ the simple open fire or furnace and water, and it is probable that with these two simple agents good cast-steel can be as successfully and properly hardened for any purpose as it can be

under any other process, and the advantage gained by heating in fluxes consists in increased expedition and the necessity for a less expert manipulation.

The splitting or cracking of steel occurs during the cooling part of the hardening process, and is to be easily avoided even with the most unfavorable of steels, if the conditions of cooling are made to conform to the form and size of the article. The cooling is, in a large majority of cases, performed by dipping the heated steel in water; and the manner in which the dipping is performed may be made at will to crack, warp, or straighten the article.

The instant the surface of a piece of red-hot steel enters the water a rapid contraction of the submerged portion takes place, and unless this contraction is kept equalized to suit the shape of the article, the side or part most contracted will bend hollow, causing the diametrically opposite metal to bend to accommodate the inner curve. Suppose, for example, we heat a piece of steel, an inch square and twelve inches long, to a red heat, and dip it slowly in water, so that one side of the square will strike the surface flat and evenly; then that surface will contract while the diametrically opposite or upper surface will remain expanded; the lower face will curve to a concave, the upper one to a convex. If, then, such a bar were curved during the heating process, we may help to straighten it by dipping it slowly in the water, with its convex side downward. If it was bent at one end only, we may dip it at that end first, diagonally, and with the convex side downward. If, however, we dip it with its length lying either diagonally or horizontally, we are apt to warp it, no matter how quickly it may be dipped, and the reason is, in addition to the above, as follows: Experiments have demonstrated that the greater part of the hardness of steel depends upon the quickness with which its temperature is reduced from about 500° to a few degrees below 500°, and metal heated to 500° must be surrounded by a temperature which renders the existence of water under atmospheric pressure impossible; hence, so long as this temperature exists, the steel can not be in contact with the water, or, in other words, the heat from the steel vaporizes the immediately surrounding water. The vapor thus formed penetrates the surrounding water and is condensed, and from this action there is, surrounding the steel, a film of vapor separating the water from the steel, which continues so long as the heat from the steel is sufficiently great to maintain that film against the pressure of the water and the power of the water which rushes toward the steel to fill the spaces left vacant by the condensation of the vapor as it meets a cooler temperature and condenses. The thickness of the vapor film depends mainly upon the temperature of the steel, but here another consideration claims attention; as the heated steel enters the water, the underneath side is constantly meeting water at its normal temperature, while the upper side is surrounded by water that the steel has passed by, and, to a certain extent, raised the temperature of. Hence the vapor on the underneath side is the thinnest, because it is attacked with colder water and with greater force because of the motion of the steel in dippings. Suppose, now, we were to plunge a piece of heated steel into water, and then slowly move it laterally, the side meeting the water would become the hardest, and would be apt to become concave in its length.

From these considerations we may perceive how important a matter the dipping is, especially when it is remembered that the expansion which accompanies the heating is a slow process compared to the contraction which accompanies the cooling (although their amounts are of course precisely equal), and that while unequal expansion can only warp the article, unequal contraction will in a great many, or, indeed, in most cases, cause it to crack or split.

After an article is dipped to the required depth, it should, if straightness is of importance, be held quite still until reduced to the temperature of the water, because, if taken out before so reduced in temperature, it is especially apt to crack; and it is better to have a deep tank of water, if the body of the metal is great, so that the steel may be dipped slowly downward and become cooled sufficiently rapidly to harden without any lateral movement, except it be after the steel has lost its redness.

When a piece of steel requires to be hardened at one end only, the dipping must be performed with a view to make the graduation from the soft to the hard metal extend over a broad section of metal; for if the junction of the hardened with the soft metal is abrupt, the hardened end is apt to break short off. The method of dipping, therefore, is, in this case, to plunge the end of the steel vertically into the water, to a depth a little more than equal to the depth it requires hardening, and, after holding it still there until it is black hot (that is, as soon as its redness is gone) dip it slowly a little deeper, and then raise it up to the amount of the increased dipping, and slowly immerse again.

When a piece of metal requires hardening and tempering at one part only, we may heat the steel back of the part to be tempered to redness, and dip the article so as to harden the required part, and leave sufficient heat in the contiguous metal to raise the temperature of the hardened part enough to temper it. This plan is always followed in the tempering of the lathe and planer tools, flat drills, &c. If, however, the method of dipping is to hold the steel in the water at an even depth after the immersion, the temper-color will be very narrow, while, if the steel is raised and lowered in the water, the color-band will be broad.—*Polytechnic Review.*

RAILWAY NOTES.

THE fact that some steel rails wear much longer than others has attracted much attention from engineers and metallurgists. The question is one of considerable importance, and for this reason different rails, which had been in use on the Pennsylvania railway, have been carefully analyzed, and their chemical composition and other qualities ascertained and compared with their wear. Dr. Dudley, chemist in the testing department of that railway, read a paper at a late meeting of the American Institute of

Mining Engineers, in which some of the results of his investigations were given. These confirm the observation often made, that the hardest rails do not wear the longest. The wear to which steel rails are subjected is that of rolling friction, which is in reality a succession of blows. The effect of blows on a hard substance is to crumble it, while on the softer materials a permanent distortion or change of form is effected, which is in reality the "flow" of the metal under the pressure of the blows. A material, then, which is so soft that it will not crumble, and so hard that it will not flow, will probably offer the greatest resistance to wear under a succession of blows like those produced by rolling friction. The question is of great importance, as uniformity in quality of steel rails would secure a great annual money saving.

It is stated that chilled wheels were used on the Emperor Ferdinand Northern railroad, of Austria, as early as 1855, and have ever since increased in number, so that now, with 10,000 luggage trucks, 23,140 such wheels — 21,696 from Ganz and Co., Buda, and 1,444 from Count Andrassy's works, at Dernoe — are in use. The following figures are deduced from a table giving some data concerning the life of chilled wheels on the Northern railroad. The time is given and not the mileage; the latter can be, however, approximately found by multiplying the years by the average mileage per year, namely, every wheel is guaranteed to last five years, and in case of failure is exchanged by the manufacturer. Between the years 1855 and 1872, 4,309 chilled wheels were bought; of these, 25,497 or 74 31 per cent. were exchanged before reaching five years of service, and up to 1878, 6,505 more, or 19.2. per cent., had been disabled, leaving but 2,307 still in use. Of the latter number, 10 have been running since 1862, 41 since 1864, 129 since 1865 and 1867, 530 since 1868 and 1869, 709 since 1870, and 888 since 1871 and 1872. In the last five years an additional number of 24,854 was bought, and of these, 5,465 have failed already. The average life of those wheels that failed before reaching the guaranteed time was 3·1 years, and the average life of wheels which exceeded five years but failed before 1878 was seven years, giving a total average life of 3.9 years for the wheels that failed. The 2,307 wheels still remaining will probably not exceed an average of 4.5 years. From 1865 to the end of 1877 only thirteen wheels have been broken.

ENGINEERING STRUCTURES.

The English Channel Tunnel.—The Channel Tunnel is still only a possibility. Nothing is being done on the English side, but on the French coast the borings are continued, and the information obtained confirms the geological evidence on the strength of which the undertaking was proposed. The scheme, however, does not meet with favor in influential circles here.—*English Mechanic.*

The Grand Canal of China.—This canal is likely to share the fate of the Great Wall. This water-way was constructed by Kublai-Khan and his successors of the Yuen race, and is 600 miles in length. There are 10,000 flat-bottomed boats on this canal, and these are used in the transportation of grain. The *Echo* states that this great water-way is an enormous "white elephant," as it costs an enormous amount every year for repairs, the appropriations there, as elsewhere, not being entirely devoted to the purpose for which they are meant. Junks are delayed every month while channels are dug for their passage. This year, for the first time since the construction of the canal, the grain from Nanking, with the consent of the government, has been forwarded by sea, and this fact has impelled the Peking authorities to consider the expediency of abandoning the canal as a commercial highway.

ORDNANCE AND NAVAL.

Determination of the Resistance Offered to Ships. — In an article contributed to the *Rivista marittima*, Signor A. Lettieri has described an apparatus for determining the resistance offered to ships by experiments on their models. The inventor considers that the determination of the resistance encountered by a vessel moving at different velocities in still water is a most important question, which has been solved by Mr. Froude. The law which this gentleman has formulated, by which to deduce the resistance met by a vessel from those encountered by its model, Signor Lettieri considers to have been fully verified by the experiments made by Mr. Froude on the *Greyhound* and its model. The further prosecution of similar experiments Signor Lettieri thinks useful, or even necessary, with the view of ascertaining, before the launch of a vessel, the curve of the resistance that it will encounter with different loads and displacements. Being unacquainted with the apparatus used by Mr. Froude, Signor Lettieri has invented one of his own, the description of which he illustrates with a drawing. In experiments of this nature the elements to be determined are two: the uniform velocity, and the resistance encountered at that velocity. The first of these is obtained by the measure of the space passed through in a unit of time. It is, therefore, desirable to have an apparatus which shall graphically denote this velocity by a curve, and refer it to a measure of the resistance. To effect this, Signor Lettieri has designed a vertical cylinder (the drawing shows the length to be fourteen times the diameter, but neither scale or dimensions are given), which revolves on a fixed axis. The upper part of this axis sustains a pulley, and a second pulley is fixed beneath the cylinder, with a small drum on its axis. A line attached to the drum passes over the upper pulley, and sustains a scale pan, to which is fixed a pencil, the point of which presses against the cylinder. The model is attached by a line to the lower pulley, so that the descent of the weight corresponds to the movement of the model through the water; while the weight itself is a measure of the resistance. Movement is given to the vertical cylinder by means of a pair of conically-

toothed wheels, one of which is attached to the cylinder itself. The motion of the latter being made thus uniform, and its velocity known, the curve traced on it by the pencil will indicate the relation between the movement of the model and that of the cylinder, and will form a regular spiral when both movements are uniform. The remainder of the paper is occupied by an algebraical investigation of the curves thus to be obtained, and by the relation between the weight placed in the scale pan, and the resistance encountered by the model in its passage through the water.

BOOK NOTICES.

OUTLINES OF MODERN ORGANIC CHEMISTRY. By C. GILBERT WHEELER. Professor of Chemistry in the University of Chicago. Chicago: Jansen, McClurg & Co.

The selection of materials for this compact little volume, from the great mass of materials at hand, has been judiciously done.

The student is supposed to be familiar with modern Inorganic Chemistry; he will then find this the best supplement to his course that he could well get, unless he intends to become a professional chemist. For all other needs of a student this work is sufficient.

The typography is excellent, the chemical formulas being exceptionally neat

THE HEMPSTEAD STORAGE RESERVOIR OF BROOKLYN: ITS ENGINEERING THEORY AND RESULTS. By SAML. MCELROY, C.E. New York: D. Van Nostrand. Price 50 cts.

This pamphlet discusses certain important questions of hydraulic engineering upon which the author has taken issue with the engineers by whom the late construction of the Brooklyn works has been directed.

As presented by Mr. McElroy, engineers will find the discussion very interesting. The estimates and promises of the projectors of the work as completed are carefully compared with the performance down to very recent date.

HYDRAULICS AND PNEUMATICS. By PHILIP MAGNUS. London: Longmans & Co. 1878. Price 75 cts.

This volume of the London Elementary Science Classbooks, by one of the editors of the series, is intended for upper-form pupils about to commence the study of a branch of physics which has an important connection with practical mechanics. The method of treatment employed is in some respects novel, the more popular course of illustration by experiment being used as much as possible; while, when it is necessary to deduce results by the help of mathematics from more elementary principles. the experimental proof is likewise superadded. The subject-matter is divided into short sections in a convenient way for the classroom, and to most of these a set of exercises is appended. At the end of the volume a series of miscellaneous problems is given. The information supplied is well brought up to date, and the diagrams and other illustrations are abundant and suitable.

RESEARCHES IN GRAPHICAL STATICS. By H. T. EDDY, C.E., Ph.D. New York: D. Van Nostrand. Price $1.50.

These "Researches" include a considerably amplified edition of two papers entitled respectively "Certain New Constructions in Graphical Statics" and "A New Fundamental Method in Graphical Statics," originally read by the author at Buffalo, in 1876, and of a third paper containing considerable new matter in problems relating to the combination of states of stress, and entitled "The Theory of Internal Stress in Graphical Statics."

In the first paper the author, who is Professor of Mathematics and Civil Engineering in the University of Cincinnati, deals largely with the various forms of the elastic arch, and has aimed by presenting, so to speak, a pictorial representation of the thrusts, moments and shears, to make clear those relations which in the analytic method are obscured by the intricacy of the required formulæ. He also gives a new investigation of the continuous girder with various sectional areas, as well as graphical solutions of many problems relating to domes of iron and masonry, the stability of retaining walls and other matters.

Professor Eddy discusses the stability of arch ribs, with and without hinge joints, by means of the "Equilibrium Polygon." In arches a special polygon appertains to a given arch and load, in which the horizontal stress is the actual horizontal thrust of the arch. The methods used to obtain the latter depend upon the separation of the stresses induced by the loading into two parts; one part being sustained by the arch in the same manner as an inverted suspension cable, that is, as an equilibrated linear arch, and the remaining in virtue of its reaction as a girder. When this thrust has been obtained, the equilibrium polygon permits of an immediate determination of all other questions respecting stresses, whether induced by load, change of temperature, or otherwise, because, as demonstrated in two places by Professor Eddy, if the equilibrium polygon and the arch itself regarded as another polygon, are so placed that their "closing lines" coincide, and their areas partially cover each other, the ordinates intercepted between these two polygons are proportional to the real bending moments acting in the arch.

As a practical example of the arch rib with fixed ends, the author takes the St. Louis steel bridge, and confirms the original calculations, and as an illustration of his graphical method applied to the elucidation of the really indeterminate strains on the stays, and stiffening girder of a suspension bridge of the Roebling type, he takes the Cincinnati and Covington Bridge of 1075 ft. span. The author appears to prefer a flexible arch rib with stiffening girder to the rigid rib universally adopted, and would anchor the girder to the piers in such a state of bending tension as to exert considerable pressure upon the arch. This in effect is an adaptation of the American system of "counterbracing" in "quadrangular" trusses to the special case of an arch, and may be advantageous in pin-jointed structures. He makes the somewhat surprising statement that the St.

Louis Bridge (525 ft. span) is wanting in "initial stiffness" to such an extent that the weight of a single person is sufficient to cause a considerable tremor over an entire span. This may appear incredible to engineers who have neither been intrusted with the building of a wrought-iron or steel arch bridge nor have watched with intelligence the proceedings of others, but will be looked upon rather as an interesting confirmation of their own experience by those responsible for the design of such structures. We know of no wrought-iron arch bridge in this country which has not exhibited this sensitiveness to vibration, and so long ago as 1868, in criticising the proposed design for the St. Louis Bridge, we predicted that it would prove unusually "lively," unless the arch rib or spandril filling was made considerably stiffer than was at that time proposed. In execution, the arch rib was made twelve feet instead of eight feet deep, but even then, owing to the intentional absence of all rigidity in the spandril filling the bridge, though enormously strong, is still to the inexperienced eye rather too sensitive to tremors. This, after all, should be no matter for surprise or alarm, since we need not go further than Westminster to find conditions of strata such that the passage of a spring van down the roadway will set thousands of tons of soil and houses into very perceptible vibration.

In the paper entitled "A New General Method in Graphical Statics," a fundamental process or method is established of the same generality as the "Equilibrium Polygon." The new method is designated as that of the Frame Pencil, and both the methods are discussed side by side in order that their reciprocal relationship may be made the more apparent.

Finally, the author, by a presentation of the subject of internal stress from a graphical point of view has, he believes, added a branch to the science of graphical statics which has not heretofore been recognized as susceptible of graphical treatment, and by which the entire investigation is brought within the reach of any one who might wish to understand it.—*Engineering.*

URE'S DICTIONARY OF ARTS, MANUFACTURE, AND MINES. By ROBERT HUNT, F. R. S., assisted by numerous contributors. Vol. IV. Supplement. Longmans, Green and Co. 1878. Price $12.00.

Scientific discoveries follow one another in these days so closely, and the application to the industrial arts takes place so rapidly, that scientific and technical dictionaries must almost immediately after their appearance be followed by the publication of supplemental parts. The volume before us is well up to date; it contains notices of the liquefaction of the so-called permanent gases, and of the telephone. In glancing over the articles devoted to the more important chemical industries, we find them, with few exceptions only, replete with the best information.

The refining of pig-iron, and in particular the elimination of the phosphorus, is a subject of much interest. Jacobi proposes to separate the phosphorus from the ore itself by breaking the latter into small pieces, calcining in vertical kilns, heating the calcined mass in tanks with a solution of sulphurous acid — obtained by burning iron pyrites and condensing the fumes in water — heating the acid solution in a coil of cast-iron pipes, and allowing the liquor to settle in tanks, where the phosphate of alumina deposits in the form of a white powder.

The application of the spectroscope for determining the precise moment, when the charge of pig-iron in the Bessemer converter has been burned into steel, induces the editor to give an epitome of the theory of the spectrum, which will prove acceptable to practical men, though we must warn them against some of his notions. The use of spectral analysis for quantitative determinations, as suggested by Lockyer and Roberts, is mentioned, though we doubt whether it is good for anything.

The articles on water and wine disclose a very dismal state of things. It appears that the so-called filtration of the Thames water by the Metropolitan companies is absolutely worthless. Prof. Frankland has demonstrated that in the river water supplied to London the soluble organic matters and some of the suspended matters of sewage and manure reach the water-drinker in a few hours, and in substantially the same condition in which they leave the sewers and the fields. Turning to the paper on wine, we learn that the manufacture of wines is now carried on as openly as any legitimate trade, sherry and champagne in particular being subject to a complicated artificial treatment. Can the adulteration act not be applied to the vendors of "plastered" and "fortified" sherry and "scented" champagne? The production of aniline without the use of arsenic must be considered a great improvement. The use of salicylic acid for antiseptic purposes seems to spread; not only beer and wine, but bread and meat may be kept by it for several weeks in good condition. In the account of the application of dynamite in the clearing of land, we miss the information supplied by Trautzel in the well-known little book of his. In sugar-refining, evaporation of the syrup by means of blowing air through the mass, has been advantageously used by E. Moride at Nantes. He succeeds in bringing the syrup to 35 and even 40 deg., without changing its color or inverting the crystalline sugar. The saving in time and fuel is considerable. The final evaporation has, however, to be effected in vacuum.

Although the larger and more complete articles in this dictionary are devoted to manufactures and mines, considerable space is devoted to mechanical arts. Among others of these noticed in this division of the supplement before us, is "Agricultural Mechanics," a title much more comprehensive than the articles that have commonly been written under it. In a dictionary we cannot expect to find a treatise on each subject, and must, therefore, overlook incompleteness in the consideration of machinery in detail, and, perhaps, to some extent, number of implements referred to under a title such as the above. But it should be complete as far as it goes. We have in this article reference to steam ploughing apparatus, mowing and reaping machinery, thrashing machines, corn elevators, chaff cutters, and pulpers.

Under the head of electric light, a clear de-

scription is given of the Siemens dynamo-electric machine, and of the Gramme machine, and the principle upon which it is constructed. The description of the former is rendered less clear by the incorrect use of capital letters, instead of italics, in the bottom line of page 339.

In dealing with rock-boring machinery, a very useful and satisfactory sketch of recent advances in this direction is given. Of wood-working machinery some excellent examples are given, but it seems hardly fair that all these should bear the name of one maker, however good. — *Abstract from Engineer.*

THE ELEMENTS OF GRAPHICAL STATICS AND THEIR APPLICATIONS TO FRAMED STRUCTURES, WITH NUMEROUS PRACTICAL EXAMPLES OF CRANES, BRIDGE, ROOF AND SUSPENSION TRUSSES, ETC. By A. JAY DUBOIS, C. E., Ph. D. New York. 1875. John Wiley & Son.

In the course of a review of DuBois' "Graphical Statics," published in the *Zeitschrift des Ver. Deutsch Ing,* the writer says:

"This surprisingly long title is followed by a preface of ten closely-printed pages, which contains notices valuable to the student while using the book. The table of contents, of twelve pages of fine print, is preceded by a four-page note. 'Elements of Graphic Statics,' intended especially for student and teacher. Then follows, under the title 'Introduction,' an excellent and exact translation (!), including references of the capital work of our German colleague, Dr. J Weyrauch, '*Ueber die Graphische Statik,' Leipsig: Verlag von Teubner.* The title of the first chapter, 'Historical and Critical,' is accompanied by an asterisk with the reference 'Weyrauch, U. S. W.;' and in his preface DuBois says: 'For the historical and critical introduction we are indebted, a few alterations excepted, to the pen of Weyrauch. It will be useful, &c., &c.' As regards the 'few alterations' of DuBois, we have not been able to discover them, except in the omission of several scientific references of Weyrauch. The American reader is led to infer from DuBois' method of reference that only one page of his 'Introduction' is taken from Weyrauch; when, in fact, as I find after a thorough examination, there are twenty-seven pages of close translation.

"What particular use was made of Culmann, Mohr, Ritter, Winkler and Reuleaux, and how much Cremona, Favaro and others were studied, after the entire literature had been collated by Weyrauch's diligence for the benefit of the translator, we shall not determine; but to DuBois belongs the credit of industry in collecting, and of the introduction of practical examples."

Such a work will find recognition among German scholars; and because of the want of such a work, long felt, his American colleagues will gladly avail themselves of its benefits; especially in view of the fact that he has made use of the very abundant literature on the subject in German, French, Italian and English, which has appeared since the first edition of Culmann's Graphic Statics, in such a way that one can obtain a comprehensive view of all the works which the subject comprises. Teacher and student are under obligations to him for having made it possible to get a clear apprehension of the controversy concerning the necessity of the use of the methods of the so-called Modern Geometry, as maintained by Culmann, in opposition to the methods of Bauschinger, who dispenses with the Modern Geometry. The author has avoided partial treatment by the introduction of the elegant methods of Von Standt, adapted by Reye to Graphic Statics, into his translation of Weyrauch's work, making its use possible to those who, already engaged in professional life, have never studied the Modern Geometry; and who have, therefore, been obliged to give preferences to the graphic methods deduced by analysis. Though the author inclines to Bauschinger's methods, which prevail in Germany, we must object to his selection of details merely from the diligent work of the German scholar, when he should have presented his elaborated material entire. For example: Bauschinger undertakes the laborious task of determining the center of gravity of a rail-section; DuBois merely gave Bauschinger's diagram — which is a masterpiece of drawing — to the lithographer, who copied it correctly, but not with the delicacy and precision of the original. Besides this, "entire plates show a lack of the care in delineation which is required in a work like this."

We would not convey the impression, by our severe criticism, that this work is not of extraordinary advantage for American students and teachers. On the contrary, we think it will be of great use to practical men, because of the numerous examples; and to the scholar, because of Weyrauch's diligent collation of authorities.

The book is recommended to German readers, especially to students, as a good exercise book in English, on account of its clearness of exposition To the American student it is especially useful, because of the careful directions given in the introduction as to the order of reading and the arrangement of material.

[NOTE. — An abstract of the above review was given in our November issue. As the author expressed some dissatisfaction with the incompleteness of the translation, in his note in the December number, we have given above the entire review. — ED.]

A TEXT-BOOK ON THE STEAM ENGINE. By T. M. GOODEVE, M.A. New York: D. Van Nostrand. (*In preparation.*)

THOSE best acquainted with the subject are aware that the English literature of the steam engine is exceedingly imperfect. A considerable number of treatises on the theory and practice of steam engineering has been written and published, but not more than half a dozen of these treatises deserve a place in a well selected library. The little volume before us may be regarded as one of the best text-books of the steam engine of its size that have yet been produced; and we say this advisedly, and giving due consideration to the writings of Lardner, Bourne, Rankine, Cotterell and Rigg.

Professor Goodeve has given us a treatise on the steam engine which will bear comparison

with anything written by Huxley or Maxwell, and we can award it no higher praise. The author's brief preface so plainly sets forth the plan of the book that we reproduce it just as it stands:—

"The first chapter contains a sketch of the steam engine as it existed in the time of Watt, together with an account of the ideas then prevalent, as to the nature of heat, and concludes with a summary of some physical properties of steam. The second and third chapters are occupied by an investigation of the principles of the modern theory of heat in its applicaton to the steam engine. Then comes a chapter on the conversion of motion, which deals with certain salient points in the mechanism of an engine, The fifth chapter is mainly devoted to the expansion of steam, to the action of valves, and to the application of Watt's indicator. The sixth chapter treats of boilers and the consumption of fuel. The seventh chapter is on compound cylinder engines, and is illustrated by some drawings of the engines constructed by Messrs. Maudslay, Sons & Field, for the White Star line of Mail steamers, making the voyage between Liverpool and New York. Finally, there is a chapter on miscellaneous details, such as steam engine governors, Giffard' s injector, and the link motion The work concludes with a series of examination questions. The author's chief object has been to point out the influence which the change in our views as to the nature of heat has excercised on the practical construction of the steam engine, and he has further endeavored to show the manner in which Watt's diagram of energy has enabled us to accomplish a scientific analysis of the action of heat engines generally, and in particular of the steam engine, under all its varied forms."

Space would fail us did we attempt to follow our author through his volume, and indicate step by step the way in which he has carried out the programme which we have reproduced above. The book is an octavo of 269 pages, and its price is so moderate that no one interested in the steam engine need be without it.

A large portion of the volume is devoted to a consideration of the laws of heat as affecting the steam and other engines, and he has actually succeeded, or we are very much mistaken, in making the theory of heat engines perfectly intelligible without having recourse to any but very simple and easily manipulated formulæ. This is in itself a considerable achievement.

As an example of this useful quality, let us take an instance at haphazard. Our author is dealing with the properties of a vapor, and he points out that a vapor, being a gas, has that property of indefinite expansion which characterizes gases. It follows that if a small quantity of vapor be formed in a closed vessel, it will expand and fill the whole of it. There are, he goes on to say, two cases to be considered: (1) when the vapor is in contact with the generating fluid, (2) when it is entirely separated therefrom. It would be very easy to write many pages considering these two cases, but our author does nothing of the kind; he cites an experiment which places the facts which the student has to learn at once before his mind's eye. We have never seen any allusion to this experiment in any other book, and we therefore reproduce our author's account of the way in which it is to be performed:—

"Take a barometer tube, say, about 33 inches long, and closed at one end. Fill it with clean mercury, which may be done by pouring in mercury nearly to the level of the open end, closing the end with the finger, and then passing the large bubble of air two or three times up and down the tube. This removes all the minute bubbles of air which adhere to the glass, and mercury may be added up to about $\frac{1}{2}$ inch from the open end; then fill this empty space with bisulphide of carbon, a very volatile liquid, and insert the tube in a deep well of clean mercury. The bisulphide of carbon will rise to the top of the tube, vapor will form in the empty space above the mercury, and will, by its pressure, drive down the column of mercury so as to shorten it considerably as compared with the column in an ordinary mercurial barometer. We have accordingly a small layer of liquid lying on the top of the mercury, and several inches of apparently empty space above the liquid. A singular result may now be exhibited. Depress the tube by the finger so as to sink it in the well or cause it to rise higher, when it will be found that the height of the column within the tube—measured from the surface of that in the well—remains absolutely constant. If the tube be raised quickly the liquid begins to boil, fresh vapor is formed instantly, and the pressure is kept at a constant intensity; on the other hand, a portion of the vapor passes into the liquid state, when the space which it fills is contracted, and nothing will alter permanently the height of the mercurial column except a permanent change in the temperature of the liquid and the tube.

No illustration of the operation of a great natural law could, we think, be happier. Hypercritics may take exception to the use made by the author of the word vapor, yet it would perhaps be impossible to find any other which would answer the required purpose equally well.

Not the least valuable portion of Mr. Goodeve's work is that in which he handles Carnot's principle, which affords a remarkable example of a great truth evolved from a wholly erroneous premise. Carnot believed that heat was a material fluid, and yet he evolved the truth that as the whole work done by a heat engine is traceable to the disappearance of heat, and to that alone, it follows that a heat engine is entirely independent of the nature of the substance with which it performs its functions. Whether the apparatus is a steam, air, or ether engine, the result is the same. The amount of work done by a reversible heat engine depends only on the constant temperatures at which heat is received and at which it is rejected, and is uncontrolled by the nature of the intermediary agent, such as steam or air. It is impossible to attach too much importance to this law. Because it has been overlooked, much money has been wasted in vain endeavors to attain an economy which cannot be had

—Abstract of a Review from Engineer.